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LIVINGSTONE AS AN EXPLORER[1]

DAVID LIVINGSTONE, it is scarcely necessary to remind you, was of Highland descent, his grandfather having been a crofter on the little island of Ulva, off the west coast of the larger island, Mull. In appearance he showed clearly that the predominant strain in his ancestry was what we call Iberian for want of a more definite word. That is to say, that he was of that very old racial strain still existing in western Scotland, western Ireland, Wales and Cornwall, which has apparently some kinship in origin with the peoples of the Mediterranean, and especially of Spain and Portugal. Indeed, according to such descriptions as we have of him, and such portraits as illustrate his appearance, he was not unlike a Spaniard, especially in youth and early middle age. His height scarcely reached to 5 feet 7 inches, his hair and moustache, until they were whitened with premature old age, were black, his eyes hazel, his complexion much tanned by the African sun, but at all times inclining to sallow. He possessed a natural dignity of aspect, however, which never failed to make the requisite impressions on Africans and Europeans alike. Bubbling over with sly humor, with world-wide sympathies, and entirely free from any narrowness of outlook, he possessed a very strong measure of self-respect, coupled with a quiet, intense obstinacy of purpose.

. . . . . .

[1] From the address to commemorate the centenary of the birth of Livingstone given before the Royal Geographical Society, London, and the Royal Scottish Geographical Society and printed in the journals of these societies.

Let us briefly consider his achievements as a geographical discover. He directly inspired the search for Lake Ngami, and was the main agent in carrying South African exploration beyond the arid plateaus of Bechuanaland and the Kalahari desert into what is really the Zambezi basin. Oswell and Murray contributed to the cost of his journeys, but he by his influence found the guides and secured the friendship or the neutrality of the native chiefs. He acted as interpreter-in-chief, and, thanks to the mastery he had acquired over the Sechuana language, was able to converse fully and freely with the natives of South-Central Africa. He also picked up a considerable knowledge of other dialects. He served diligently and skilfully as physician and surgeon all who were connected with these journeys. But his own predilections were for botany, zoology and the study of man. It was the impression that native reports of his character had made on Sebituane, the Makololo conqueror of the upper Zambezi, and the resultant protection afforded, which made it so easy for Livingstone and Oswell to reach the Chobe River and the upper Zambezi in 1851.

Between 1852 and 1856, Livingstone traced the main course of the Zambezi from its confluence with the Chobe northwards to near the sources of the Liba, and from this point westwards he was the first scientific geographer to lay down correctly the position of the upper Kasai and Kwango affluents of the Congo.

Livingstone may be quoted as the discoverer of the great Kasai (perhaps the principal among the Congo affluents for volume and for extent of drainage area). At first it would seem probable that the Pombeiros, at the beginning of the nineteenth century, must have crossed the Kasai in order to reach the court of the Mwata Yanvo. But they appear to have deflected their route southwards, after leaving the upper Kwango, so that they pass round the sources of the Kasai, leaving them to the north. Ladislaus Magyar, the Hungarian explorer and trader (who married a negress of Bihé and traveled over Angola between 1849 and 1864), penetrated about 1851 to the upper Kwango and the northwest limits of the Zambezi basin, and may have seen the infant Kasai in 1855, a few months before or after Livingstone passed by. But he did not communicate the information to the world until after Livingstone's journey, and never, I think, specifically mentioned the Kasai, at any rate, before the publication of Livingstone's book. Moreover, he was no trained geographer or taker of observations for fixing points of latitude and longitude. Silva Porto, a Portuguese trader of Bihé, reached the upper Zambezi and South Congoland in the fifties and sixties, but his wanderings resulted in no additions to the map of Africa.

It is, indeed, remarkable what Livingstone's predecessors missed rather than what they found. Dr. Lacerda reached to little Lake Mofwe, an isolated lagoon about 20 miles south of Mweru and a short distance east of the Luapula. Yet apparently neither he nor any member of his expedition, before or after his death, had the curiosity to penetrate northwards one day's journey and discover Lake Mweru, or visit the banks of the Luapula. Going through the Bisa country they heard of a lake—"Lake Chuia," or Shuia, a short distance to the westward, and knew that the Chambezi flowed into it. This was Livingstone's Bangweulu (named, as he tells us, from one of its islands). But the Portuguese of Lacerda's mission, like those of the Monteiro-Gamitto expedition of 1831–32, made no effort to locate Bangwenlu and place it definitely on the map. Lake Nyasa was

heard of (as "Nyanja") by the Portuguese of the eighteenth century and early nineteenth; but it was not till 1846 that its waters—so far as historical records go—were actually seen by a Portuguese (Candido de Costa Cardoso). Gasparo de Bocarro passed near to Lake Nyasa in 1616 on his way to Kilwa and Mombasa, but seems to have crossed Lake Malombe or the upper Shiré only, and not actually to have seen Lake Nyasa.

Returning from Angola to the Chobe River, he discovered the Victoria falls, and followed the Zambezi more or less closely down to its delta, emerging on the seacoast at Quelimane.

On his second Zambezi expedition he revealed to the world Lake Nyasa, Lake Chilwa (miswritten Shirwa), the high mountains of the Shiré region, and the course of the Shiré River, the Luangwa River to the west of Lake Nyasa, most of the northern confluents of the Zambezi in their lower courses, and the Butonga highlands. This second expedition was also the means of effecting a great increase in our knowledge of the Zambezi delta.

On his third great African journey he renewed previous explorations in the direction of the Ruvuma, and traced a good deal of the course of that East African river. He was practically the first European to explore West Nyasaland and the northern Bemba or Awemba country; he discovered the south end of Tanganyika, and made a shrewd guess at its outlet through the Rukuga (which river he styled the Longumba). He first revealed the great Mweru swamp or Chisera. ["Elephants, buffaloes and zebras grazed in large numbers on the long sloping banks of a river or marsh called Chisera." This considerable extent of alternate swamps or shallow water was afterwards rediscovered by Sir Alfred Sharpe.] Livingstone made known to us lakes Mweru and Bangweulu and the connecting Luapula River, and the course of the great Lualaba or upper Congo at Nyangwe. He also recorded the existence of the upper Lualaba or Kamolondo. He was the first European to penetrate as far north as S. lat. 3° 30′ near the Elila River, and describe the Manyuema forests with the large chimpanzis and pygmy elephants found in them. He mentions for the first time the Lomami River, and is the first explorer to hear of the country of Katanga, its mineral wealth and its—as yet—unexplored, inhabited caverns of vast size.

> A month to the westward of Kazembe's country lies Katanga, where the people smelt copper ore (malachite) into large ingots shaped like the capital letter I, weighing from fifty to a hundred pounds. The natives draw the copper into wire for armlets and leglets. Gold is also found at Katanga.

Livingstone was the first writer to mention the possible existence of Lake Kivu; of Kavirondo gulf (Victoria Nyanza); and of Lake Naivasha: from Arab information, of course.

He was the first to record the existence of drilled stones in the country to the southwest of Tanganyika, which seemed to be evidence of the existence of a people of ancient Bushmen culture in that direction, and his remarks generally on the Stone Age in Africa, on the possible existence of undiscovered ancient types of mammals and of mammalian fossils, all show an enlightenment in speculative scientific imagination greatly in advance of his times. He was also in all probability the first writer since the Portuguese chroniclers of the sixteenth century to allude to the remarkable ruins of stone-built forts, villages and cities in southeast Africa. He derived his information from natives, and perhaps also from Boer hunters. He also mentions the coins found in excavating the shore of

Zanzibar Island, with Kufic inscriptions, and perhaps dating back to the ninth or tenth century A.D. (Sir John Kirk confirms this statement, and adds that some of these coins were of Harun-ar-rashid's reign, and bore the name of his viziers, Yahya or Fadl.)

His biblical studies drew him into Egyptology, and one of his incentives to the exploration of the Nile sources was the conviction that Moses when living in Egypt had taken a great interest in Nile exploration. Livingstone half hoped that in discovering the ultimate sources of the Nile he might come across archeological traces of Egyptian influence. He was not pursuing in this direction an absolute chimera.

The physical appearance of so many of the Bantu tribes between Lunda, on the southwest, and Manyuema, Bambare and Buguha, on the northeast, constantly suggested to Livingstone's mind the idea of an immigration of Egyptians into Central Africa. Had he lived to penetrate to the countries north of Tanganyika to see the Hima or Tusi aristocracy on the highlands of equatorial Africa, he would have been still more convinced of the ancient inflow of Egyptian influence into these regions: though it is a theory which it is very unsafe to pursue on the scanty evidence we possess at the present time.

When traveling from Tanganyika to Mweru in 1869, he remarks on the appearance of the chief and people of Itawa.

> Nsama, the chief, was an old man with head and face like those sculptured on the Assyrian monuments. . . . His people were particularly handsome, many of the Itawa men with as beautiful heads as one could find in an assembly of Europeans. Their bodies were well shaped, with small hands and feet—none of the West Coast ugliness—no prognathous jaws or lark heels.

There is another entry in his journal derived from Arab information which bears on this theory of the Hamitic permeation of Negro Africa.

> The royal house of Merere of the Basango [northeast Nyasaland] is said to have been founded by a light-colored [Hamitic?] adventurer, who arrived in the country with six companions of the same race. Their descendants for a long time had straight noses, pale skins and long hairs.

His journeys into southern Congoland threw a very interesting light on a native kingdom made famous by the earlier Portuguese explorations—that of the Kazembe of Lunda, whose capital was between lakes Mweru and Bangweulu.

In the early seventeenth century a great negro empire had arisen in southern Congoland, partly due, no doubt, to the arms and trade goods derived from the Portuguese, but partly also to the after-effects of the Sudanese civilization of Central Congoland under the Bushongo dynasty. This empire of Lunda ruled over all the south of Congoland and a small part of northern Zambezia.

In the early eighteenth century a member of the family of the Lunda emperor, or "Mwata Yanvo," moved to the south of Lake Mweru and founded a feudatory kingdom there. He received the title of Kazembe or "lieutenant."

Kazembe's capital was by the side of a little lake called Mofwe. Livingstone approached it along a path as broad as a carriage road one mile long, the chief's residence being enclosed by a wall of reeds 8 or 9 feet high and 300 yards square. The innermost gateway was decorated by about sixty human skulls, and had a cannon, dressed in gaudy colors, placed under a shed before it. This, no doubt, was a gift from the Portuguese. Kazembe himself had a heavy, uninteresting countenance, without beard or whiskers, somewhat of the Chinese type, his eyes with an outward squint. He smiled but once during the day, yet that was pleasant enough, though

the cropped ears of his courtiers and the human skulls at the gate made Livingstone indisposed to look on him with favor. Kazembe was usually attended by his executioner, who wore a broad Lunda sword under his arm, and a scissor-like instrument at his neck for cropping ears. This was the punishment inflicted on all who incurred the Kazembe's displeasure.

Kazembe sat before his hut on a square seat placed on lion and leopard skins.

> He was clothed in white Manchester print and a red baize petticoat so as to look like a crinoline put on wrong side foremost. His arms, legs and head were covered with ornaments, and a cap made of various-colored beads in neat patterns. A crown of yellow feathers surmounted his cap. His head men came forward, shaded by a huge ill-made umbrella and followed by dependents. . . .

This Central African monarch (whose descendant was finally deposed for cruelties by the British government) bore an evil reputation; yet he was a good friend to Livingstone and put no obstacle in his path; though he politely told him that lakes and rivers only consisted of water, and that to ascertain this fact by ocular inspection would not repay him for his fatigues and outlay in trade goods!

Livingstone from boyhood had taken a great interest in botany and in the appearance of trees and flowers in the landscape. His observant glance led him to note all the more salient features of the African flora from the Cape to the equatorial forests of Manyuema. His books are full of little word-pictures of the strange, stately or beautiful trees and plants he encounters. He records in his journal the spectacle of the Crinum "lilies" of the Luangwa valley, which in the first rains "flower so profusely that they almost mask the rich, dark, red color of the loamy soil, and form a covering of pure white where the land has been cleared by the hoe." The weird stone- or pebble-like Mesembryanthemums of the Kalahari Desert, and the gouty, leafless geraniums and vividly colored pumpkins and gourds of the same region arrest his attention; the Bauhinia bushes with their golden or bluish tinted, bifid leaves, and the scale insects on them exuding a sweet manna; the noble giraffe-acacia trees, the euphorbias of very diverse modes of growth, the *Strophanthus* creepers whose seeds possess medicinal or violently poisonous qualities, the borassus and hyphæne fan-palms, the wild date, and the "noble raphias," the pandanus and dracænas of the Zambezi delta or of inner Congoland, the innumerable forest trees of northern Zambezia and southern Congoland: all are illustrated in his pages by well-chosen words and sometimes explanatory drawings; and most are correctly named, in contrast with the very unscientific nomenclature of the generality of travelers in his day.

Livingstone notices as he descends the slopes of the mountains towards the Chambezi the abundance of the fig-tree which yields the bark-cloth, so that the natives cared little for the cotton cloths of Europe and India. He also in this region observed green mushrooms, which, on being peeled, revealed a pink fleshy inside (the *Visimba* of the natives). Only one or two of these mushrooms were put into a wooden mortar to flavor other and much larger kinds, the whole being pounded up into a savory mess, which was then cooked and eaten. But in Livingstone's experience this mushroom diet "only produced dreams of the by-gone days, so that the saliva ran from the mouth in these dreams and wetted the pillow." The country on the Chambezi slope of these Muchinga mountains was devoid of game, the game having been killed out by far-reaching and long-continued drives through the hopo fences into pitfalls.

He crossed Tanganyika to resume his

search for the Lualaba in July, 1869. In his journal he recorded the abundance of pandanus screw pines off the west coast of that lake. As he traveled through the Guha and Manyuema countries he entered "the land of gray parrots with red tails" ["to play with gray parrots was the great amusement of the Manyuema people"]. The Manyuema country he describes as "surprisingly beautiful, palms crowning the highest heights of the mountains, and climbers of cable size in great numbers hanging among the gigantic trees." Strange birds and monkeys were everywhere to be seen. The women went innocently naked; and the Adams of this Eden wore nothing but a small piece of bark cloth. Both sexes atoned for their absence of clothing by having their bodies tattoed with full moons, stars, crocodiles, "and devices recalling Egyptian hieroglyphics." Yet although their country—prior to the Arab raids—seemed an earthly paradise, small-pox came every three or four years to Manyuemaland and killed many of the people.

It was in the Manyuema country that he came into contact with the large chimpanzi (*Troglodytes schweinfurthi*) of eastern equatorial Africa, whose range extends from the Welle-Mubangi River and Unyoro to the eastern bend of the upper Congo and the west coast of Tanganyika.

The soko, as he called this large chimpanzi, always tried to bite off the ends of the fingers and toes of the men with whom it fought, not otherwise doing them any harm. It made nests, which Livingstone described as poor contrivances with no architectural skill.

The Manyuema told him, however, that the flesh of the soko was delicious; and Livingstone thinks that through devouring this ape they may have been led into cannibalism. The sokos gave tongue like foxhounds; this was their nearest approach to speech. They also laughed when in play, and in their relations with the natives were quite as often playful as ill-tempered. The lion, which seemingly existed in the Manyuema country in spite of the forest, was said to attack and kill the soko, but never to eat him. The sokos lived in monogamous communities of about ten. Intruders from other camps were beaten off with fists and loud yells. If one tried to seize the female of another, the remainder of the party united to box and bite him. The male often carried his child, relieving the mother occasionally of her burden.

Rhinoceroses were shot in the Manyuema country. He also alludes to the pygmy elephant of Congoland, "a small variety, only 5 feet 8 inches high at the withers, yet with tusks 6 feet 8 inches in length"; and notes the killing of an elephant with three tusks, one of them growing out through the base of the trunk. [The pygmy elephant (*Elephas africanus pumilus*) of the equatorial Kamerun-Congo forests, was only rediscovered in the early part of the twentieth century.]

Livingstone was almost an expert in geology and petrology. He felt the keenest interest in the records of the rocks, and fully realized the importance of paleobotany. When descending the valley of the Central Zambezi in 1856 he discovered fossil remains of *Araucaria*, or of conifers now confined to South America and Australasia; and fully realized what his discovery meant in regard to ancient land connections between South Africa, India and South America. He was much impressed with the probable coal-bearing strata of sandstone throughout the Ruvuma Valley. A great many pieces or blocks of silicified wood appeared on the surface of the soil at the bottom of the slope up the plateaus. He wrote:

This in Africa is a sure indication of the presence of coal beneath.

In the sands of some of the rivers pieces of coal were quite common. He originated the theory of the rift valley of Lake Nyasa.

It looks as though a sudden rent had been made so as to form the lake and tilt all these rocks nearly over [namely, in the direction of Ruvuma].

His observations would seem to show that the level of Lake Nyasa was once about 55 feet above its present high-water mark. It is possible that at this high level its overflow of waters first of all passed into the basin of Lake Chilwa, and then flowed northwards into the Ruvuma system.

. . . . . .

Stanley, by relieving him when he did, gave him at least two more years of life, a certain measure of happiness, and the sweet consolation that he was not forgotten, and that the magnitude of his discoveries was appreciated. In this brief sunset glow of his life he turned his face once more towards Lake Bangweulu in order to trace the course of the Luapula to Mweru, and its junction with the Lualaba, half hoping that he might then travel down the broad stream till he entered the Bahr-al-Ghazal or the Albert Nyanza; but, although he now possessed comforts he had long lacked, and faithful, comparatively disciplined men, his strength gave out under constant exposure to rain, and to soakings in crossing rivers and marshes. Severe hemorrhage set in from the bowels, and he died of exhaustion at Chitambo's village in the swamps near the south shore of Bangweulu on May 1, 1873.

This brief record of his achievements and his sufferings may fitly close with an extract from his last journals, showing that he died a martyr to that form of religion which we call science:

In this journey I have endeavored to follow with unswerving fidelity the line of duty. . . . All the hardship, hunger and toil were met with the full conviction that I was right in persevering to make a complete work of the exploration of the sources of the Nile.

HARRY H. JOHNSTON

LECTURE AND RECITATION METHODS IN UNIVERSITY INSTRUCTION

A REFERENCE to the systems of instruction in Germany, Canada and the United States will most readily bring out the relative value of the lecture and recitation methods in university instruction. In the German and many of the Canadian universities the popular method of giving instruction consists in delivering a series of lectures and following these by a rigid final examination, while in the United States the daily recitation of textbook assignments is much more common than in the former countries. In Canada the term lecturer is frequently applied to a regular member of the faculty, but in the United States it is seldom heard except when applied to a temporary member of the university staff, who has been engaged to give a course of lectures on some special subject, and a college professor is more frequently regarded as a teacher than as a lecturer.

A student, therefore, who has been familiar with the German system or who has spent some time in certain departments of the Canadian universities, and then enters almost any of the American institutions of higher learning, will be impressed by the difference in the methods of instruction in these various countries. He has been familiar with the freedom of the lecture system where he is in a large measure placed on his own responsibility and, as a rule, may attend lectures or not as he chooses. Under this system he is inclined to feel that the professor has no concern regarding his success or failure beyond the duty of providing him with the intellectual substance of the lecture course in the best form and exacting a pound of flesh on the final examination. This notion is, as a rule, due to the lack of intimate contact between the in-

structor and the students because lecture classes are frequently very large. It is also often erroneous because the student is surprised to find later how familiar a discerning lecturer has become with his characteristics, from seeing him day after day in the lecture hall.

The fact that the student feels free during the year to follow his own desires, but at the same time knows that the responsibility for his conduct falls upon his own shoulders, has much in its favor for most students, although it is detrimental to some. In the better class of students it develops a certain independence of action and thought which better fits them for the responsibilities they must assume after leaving college, while in the students who are weak morally it often cultivates undesirable habits which lead to excesses. In Germany, where the university students are left almost entirely to themselves during the first two years of their academic life, the results of absence of restraint are expressed in the large amount of social immorality which is said to exist among German students. No doubt this freedom in college life is largely responsible for the conditions which led Bismarck to say that one third of the German students are never heard of, one third "gehen zum Teufel" and the other third rule Germany.

An objection to the lecture-examination system which is frequently raised, is that too much stress is laid on the final examination. It is said that good students often do not do themselves justice because of undue mental and physical strain and that the work is neglected until the last few weeks or months of the term, then crammed up for the final examination and immediately forgotten. It is no doubt true that most students can make a much better showing on daily recitations than when called upon to discuss a whole term's work, but, after all, the test which tries out a man for the larger spheres of his future life, is not that of holding enough matter in his mind to recite one day's assignment, but rather at the end of a term, or year to be able to sum up and round out the whole season's work. The greater benefits derived from a college course consist not in the abundance of details remembered from the text-books—because that is necessarily small—but rather in the inspiration for achievement and the capacity for work which a man develops by studying under enthusiastic and capable instructors. In preparing for the final examinations, during the last few weeks or months of the year, the student cultivates the ability to do the greatest amount of work in the least possible time, and a man now connected with a large commercial firm in Chicago once remarked to the writer that the best training for his future business career which he ever received was in preparing for the severe final examinations in one of the universities where the lecture-examination system is used.

There are certain features in the lecture system which are of advantage in the development of the instructor. To lecture properly a professor must have his subject well in hand, and, on the whole, he requires a much broader grasp of it than he who teaches from a text-book. While only few men make first-class teachers, I believe there are many men who might pass as teachers who would be complete failures at lecturing. A lecturer, as a rule, aims to eliminate all material not essential to the proper exposition of his subject, and when the subject is properly presented the student carrying a heavy course absorbs and remembers the outstanding features of the subject without having them obliterated by a great mass of unnecessary text-book details. It may be said that when the material is thus presented the student neglects collateral reading and the charge may be made that students educated under the lecture system are deficient in this line of study because the lectures usually contain the information demanded by the examination papers. If the lecture course is properly conducted, however, it is easier to require collateral reading than when a text-book is used, because the student may become familiar with the text assigned, but it is difficult to persuade him to go beyond its covers.

While the lecture system is employed in many of the universities and colleges of the United States the daily recitation of assigned

exercises is the more popular system of teaching. This system has factors in its favor as well as some objections. The reason why this system is more popular in this country is, no doubt, due primarily to the democratic nature of all institutions. There is a tendency to bring the instructors in the classroom nearer to the students and to do away with the aloofness in which English or German professors hold their students, and while this has certain advantages it also often has disadvantages in lowering the faculty from a dignified position in the eyes of the students. Along with this condition there exists a popular sentiment that many students should be graduated from the state institutions whether they wish to go through college or not. It is not at all to be inferred from this statement that all these state institutions have low standards of scholarship, but rather that a student who is continually coached along by an instructor will finally graduate who never would graduate if left to his own resources. It is believed, however, that a rigid examination system would cull out a lot of these men and that a lower percentage of mentally poor students would graduate under such a system. Another reason suggested for the popularity of the recitation method in this country is the fact that many of the students entering college are younger and less mature, from an academic standpoint, than in some other countries and they therefore require more personal assistance from their instructors. The greatest factor in favor of the recitation method—and there are those who regard this of sufficient importance to offset all objections—is that if a teacher be at all efficient even a slow student must get a fairly clear idea of the subject studied. On the other hand, the recitation is liable to degenerate in colleges to a grade-school-system of instruction. Where daily recitations are required the student who recites satisfactorily is quite justified in demanding that the results of these responses play an important part in the calculation of his credits and the result is that many instructors spend too much of their time grading the students instead of giving them the fundamentals of the subject. If the professor requires recitations on an assigned exercise he must demand that every student respond to a satisfactory degree, because a student's failure to recite for several successive periods has a demoralizing influence on the class as a whole. The professor is therefore obliged to resort to some means of forcing an inefficient student to prepare his work, and in many institutions rash criticisms or severe sarcasm are employed even in dealing with mature postgraduate students. Influences of this sort tend to lower the dignity of the faculty, and, as a rule, little is gained, because a man who will not do his best in college, at least after he has passed his first year, is seldom of much use after he leaves the institution. If he be doing his best nothing more should be expected of him; the results of his work reflect upon himself. The system of questioning students is being carried to such an extreme in this country that text-books are sometimes issued which contain the interrogatives, "why" and "how," inserted in the body of the text, thus taking away from the instructor the credit for having enough ability and initiative to ask necessary questions and develop the subject as a teacher with any enthusiasm or knowledge of the subject might wish to develop it. Fortunately such texts are designed chiefly for use in the grade schools.

In conclusion it may be said, as a result of observations in the different countries mentioned, that the chief objections which can be made to the lecture and rigid examination system are that it is hard on the weak student and gives the profligate too much freedom. As a rule it cultivates a greater independence of action and at the same time places the faculty in a more dignified position in the eyes of the students. It is the best system for imparting a broad knowledge and promoting enthusiasm for a subject.

The recitation of assigned text-book exercises assists the student who is mentally weak, as he can thus get a clearer grasp of the subject. This method is the most desirable for teaching subjects in which reference is made to a large amount of technical details, and it

is no doubt better for the average student during his first year in college, since his actions may be more carefully observed by the members of the faculty.

To offset the tendency of many young students who are away from home for the first time, to be led astray by the freedom and glamor of college life, some American colleges and universities have adopted the Freshman Adviser system. With this scheme the freshmen are divided into groups and each group assigned to some member of the faculty, preferably an instructor of experience, who acts as a counsellor and adviser to the students in his charge. If the paternal interest be wisely exercised many students may be started right in college life and succeed where otherwise they would fail, and the system becomes a great help to students and faculty. There is often a tendency, however, in the smaller institutions, for the advisers to pamper the students and make them as dependent as they would be in a preparatory school. The writer would, therefore, recommend this system, in a properly restricted form, as an excellent addition to the administration of a college or university in which the freedom of a lecture system permits the ignorant and immature student, away from guardianship for the first time, to develop bad habits which lead to undesirable excesses and a careless attitude towards his academic duties.

E. S. Moore

THE KAHN FOUNDATION

"The Kahn Foundation for the Foreign Travel of American Teachers" has issued Vol. I., Nos. 1 and 2, of its "Reports." The benefaction is unique enough to be interesting, and broad enough to invite debate, if not criticism. The deed of gift, dated January 6, 1911, was executed by Mr. Albert Kahn, of Paris, who had already founded *bourses de voyage* in France, Germany, Japan and England, and who contemplated similar action in Russia, China "and elsewhere." The trustees are Mr. Edward D. Adams, of New York City, or a successor to be nominated by the founder or by his personal representatives; the President of Columbia University; the President of the American Museum of Natural History; the President of Harvard University; the Secretary of the Smithsonian Institution; or their respective successors. The purpose, which seems to be somewhat indefinite, is stated as follows:

> For each year . . . the trustees shall in their discretion select two or more American teachers, scholars or investigators . . . preferably from the professors of such American colleges or universities as the trustees may from time to time designate . . . who will enter into and appreciate the spirit of the foundation and look upon their travels as preparation for the performance of high duties in the instruction and education of the youth of their country, and not as affording a mere vacation or pleasure trip. . . . The founder suggests that the itinerary of such travelers shall be regulated by the trustees in their discretion and shall, if deemed practicable, involve an absence from America of at least one year and include the various countries of Europe and Egypt, India, China, Japan, Ceylon and Java. The founder further suggests that each recipient of a "bourse de voyage" shall agree to furnish to the trustees a report containing the impressions and results of his travels, which report shall not exceed fifty printed pages. The reports of these travelers may be published by the trustees . . . or by the founder, in Paris, at his own expense.

The benefaction amounts at present to $3,000, and four fellows have been appointed.

In 1911–12, the beneficiaries were Dr. Francis Daniels, professor of Romance languages in Wabash College, and Dr. J. H. T. McPherson, professor of history and political science in the University of Georgia; for 1912–13 they are Dr. William E. Kellicott, professor of biology in Goucher College, and Dr. Ivan M. Linforth, assistant professor of Greek in the University of California. The "Reports" before us are those of Drs. Daniels and McPherson. Dr. Daniels carried out the letter of the deed, going *via* Great Britain, Europe and Egypt to the Orient, visiting Ceylon, India, the Malay Peninsula, Hong Kong, China and Japan, returning thence to San Francisco. Dr. McPherson confined himself to Europe, with the purpose of acquaint-

ing himself with the methods and *status* of instruction in his own field, and of informing himself on "the progress of public opinion in regard to international arbitration." The report of Dr. Daniels is a most interesting series of "reactions" to the endless succession of scenes and people that passed before him. It is notable for its tart remarks on the Germans, for its complimentary references to the English, and for its conclusions, which run thus:

> Three facts particularly struck me in my journey through the Orient. The first is the political and industrial might of Great Britain. . . . The second fact is that the Asiatics are more highly civilized than one dreams of before making the journey. . . . The third fact is the supremacy of American influence in China and Japan.

Dr. McPherson devotes more attention to educational affairs, but, like Dr. Daniels, has some hard words for the Germans. He found Paris the most profitable place he visited, and he says:

> If I shall ever be so fortunate as to have a year to devote to historical study abroad, I shall prefer Paris to either Berlin or Oxford.

More to the point, perhaps, is his final question.

> And now it is only fair to face the question, what assurance can I give the trustees that as a consequence of my year abroad the purposes of the trust and of the founder are to be promoted?

He interprets these purposes as two—"to promote the cause of civilization by rendering the beneficiaries better qualified for . . . their high duties . . . and to further international comity by helping to dispel provincial prejudice and ignorance." In other words, he infers that research is no principal object of the Kahn Foundation. In this he is probably right. For the arc of travel contemplated by the founder would appear to place the residence necessary for investigation out of the question. On the other hand, American college professors, as a class, are so accustomed to European travel in any case, that there would not seem to be much need for fresh stimulus in this direction. It might be suggested, therefore, that, if the foundation is to fill a distinct niche, the objective should be to enable American teachers to acquaint themselves with the Orient. The undoubted influence of the United States upon Japan in the past, and her growing influence in China today, to say nothing of the Philippine situation, might well serve to indicate to the trustees this more definite idea of the main purpose of the benefaction. As matters stand, the aim is too nondescript to induce confidence.

R. M. W.

THE NATIONAL CONFERENCE COMMITTEE

THE sixth conference of the National Conference Committee on Standards of Colleges and Secondary Schools was held at the rooms of the Carnegie Foundation, 576 Fifth Ave., New York, N. Y., on February 19, 1913. The following delegates were present, representing the organizations indicated:

Headmaster Wilson Farrand, Newark Academy, representing the College Entrance Examination Board,—*Vice-president.*

Professor Frank W. Nicolson, Wesleyan University, representing the New England College Entrance Certificate Board.

Dean Herman V. Ames, University of Pennsylvania, representing the Association of Colleges and Preparatory Schools of the Middle States and Maryland.

Principal Frederick L. Bliss, Detroit University School, representing the North Central Association of Colleges and Secondary Schools.

Dean Frederick C. Ferry, Williams College, representing the New England Association of Colleges and Preparatory Schools,—*Secretary-treasurer.*

Secretary Clyde Furst, as substitute for President Henry S. Pritchett, representing the Carnegie Foundation for the Advancement of Teaching.

Dr. Kendric C. Babcock, specialist in higher education in the National Bureau of Education, as substitute for Hon. Philander P. Claxton, the United States Commissioner of Education.

Dr. George E. MacLean, president of the committee from its establishment in 1906 until 1912, was present by special invitation. The National Association of State Universities and the Association of Colleges and Preparatory Schools of the Southern States were not represented at the conference. The meeting was called by Vice-president Wilson Farrand, who presided at both the morning and the afternoon sessions.

A sub-committee reported the results of an investigation of the use of the terms "honorable dismissal" and "statement of record," which it had made by individual conference and by means of a questionnaire sent to eighty colleges and universities. This report stated that there was a general agreement among the colleges and universities as to the desirability of a standardization of these phrases, as to the acceptance of a student's freedom to continue in the institution issuing the transfer papers as the criterion for the granting of such papers, and as to the great advantage of entire frankness of statement in the issuance of them. The report contained also a resolution defining the proper use of these terms which, after slight modification, was adopted in the following form:

Resolved, That the term "honorable dismissal" should be used to refer to conduct and character only, and that honorable dismissal should never be given unless the student's standing as to conduct and character is such as to entitle him to continuance in the institution granting the dismissal. Furthermore, there should in every instance be given, in the statement of honorable dismissal, full mention of any probation, suspension or other temporary restriction imposed for bad conduct, the period of which restriction is not over when the papers of dismissal are issued.

That the term "statement of record" should be used to refer to the recorded results of a student's work in the classroom, and that this statement should in every instance contain all the important facts pertaining to the student's admission, classification and scholarship. In particular, no partial or incomplete classroom record (for example, with failures omitted) should ever be given without clear evidence that it is partial or incomplete; if the student's scholarship has been such as to prevent his continuance in the institution issuing the statement of record or to render him subject to any probation, suspension or other temporary restriction, the period of which is not closed at the date of the record, a plain statement of any and all such facts should be included; and such information should be given as will make clear the system of grades employed, the number of exercises per week devoted to each course, etc.

The same sub-committee presented a review of some of the difficulties found in the application of the definition of the unit adopted by the committee at its meeting of October 9, 1909, and proposed a resolution providing for the addition of a paragraph to the explanatory statement then formulated. This resolution was adopted so that the entire definition of the unit now stands as follows:

A unit represents a year's study in any subject in a secondary school, constituting approximately a quarter of a full year's work.

This statement is designed to afford a standard of measurement for the work done in secondary schools. It takes the four-year high-school course as a basis, and assumes that the length of the school year is from thirty-six to forty weeks, that a period is from forty to sixty minutes in length, and that the study is pursued for four or five periods a week; but, under ordinary circumstances, a satisfactory year's work in any subject can not be accomplished in less than one hundred and twenty sixty-minute hours or their equivalent. Schools organized on any other than a four-year basis can, nevertheless, estimate their work in terms of this unit.

A four-year secondary school curriculum should be regarded as representing not more than sixteen units of work.

An ambiguity in the interpretation of the definition of the admission Latin requirement announced by the Commission on College Entrance Requirements in Latin in October, 1909, having been brought to the attention of the committee, it was decided to send communications to the chairman of that Commission, to the American Philological Association, and to the College Entrance Examination Board asking that steps be taken to remove the difficulty by an authoritative pronouncement on the subject.

The committee considered the question of the assignment of unit values to the new definition of the admission requirement in English and voted that, as a tentative arrangement, equal values be given to (1) the grammar and composition, and (2) the reading.

Among the questions assigned to a sub-committee for consideration and report at the next meeting are the following: the literal interpretation of the definition of the unit; the greater unit value of the work of the latter years of the secondary school curriculum as compared with the work of the earlier years; the effect on the unit value of work in any subject when it is accompanied or preceded by work in allied subjects; the assignment of unit values to the definitions of the admission requirements in the subjects, algebra, English and history; and the accrediting of candidates for admission to college from secondary schools which give instruction in only one foreign language.

Officers were elected for the year as follows: *President,* Headmaster Wilson Farrand, Newark Academy; *Vice-president,* President A. Ross Hill, University of Missouri; *Secretary-treasurer,* Dean Frederick C. Ferry, Williams College.

The sub-committee which had served for the past two years was continued for investigation and report at the next meeting. This committee includes Headmaster Wilson Farrand, Dean Frederick C. Ferry, President Henry S. Pritchett and Principal Frederick L. Bliss.

The next conference was appointed for February, 1914, or for such earlier time as the sub-committee might select.

FREDERICK C. FERRY,
Secretary

THE DANA CENTENARY

IN commemoration of the great geologic work of James Dwight Dana, Yale University proposes to hold a centenary celebration next November, to consist of a series of lectures culminating in a Dana Memorial volume on "Problems of American Geology." The lectures will be given on the Silliman Foundation, the dates to be announced after the opening of the next collegiate year. The lecturers and their respective subjects are as follows:

PROBLEMS OF AMERICAN GEOLOGY

Introduction

"The Geology of James Dwight Dana," Professor William North Rice, Wesleyan University.

I. *Problems of the Canadian Shield*

"The Archeozoic and its Problems," Professor Frank Dawson Adams, McGill University.

"The Proterozoic and its Problems," Professor Arthur Philemon Coleman, University of Toronto.

II. *Problems of the Cordilleras*

"The Cambrian and its Problems," Dr. Charles Doolittle Walcott, Smithsonian Institution.

"The Igneous Geology and its Problems," Professor Waldemar Lindgren, Massachusetts Institute of Technology.

"The Tertiary Structural Evolution and its Problems," Dr. Frederick Leslie Ransome, United States Geological Survey.

"The Tertiary Sedimentary Record and its Problems," Dr. William Diller Matthew, American Museum of Natural History.

SCIENTIFIC NOTES AND NEWS

PRINCETON UNIVERSITY has conferred the degree of doctor of science on Dr. Simon Flexner, scientific director of the Rockefeller Institute for Medical Research and on Dr. David L. Edsall, Jackson professor of clinical medicine in the Harvard Medical School. The degree of master of arts has been conferred on Mr. William Barnum, editor of the publications of the Carnegie Institution.

THE Jefferson Medical College, Philadelphia, has conferred the degree of LL.D. on Dr. Abraham Jacobi, emeritus professor of diseases of children, College of Physicians and Surgeons, Columbia University, and on Dr. Francis P. Venable, president of the University of North Carolina.

DR. E. A. SCHÄFER, professor of physiology in the University of Edinburgh, has received the honor of knighthood. The same honor has been conferred upon Professor J. H. Biles, professor of naval architecture in the University of Glasgow.

The Royal Dutch Geographical Society at Amsterdam has elected Professor W. M. Davis to honorary membership as "master in the art of organizing excursions in the old and the new world, and ingenious founder of a new system of geomorphology."

The Portugal Academy of Sciences has elected Dr. L. A. Bauer a corresponding member.

Admiral Robert E. Peary lectured before the French Geographical Society on the evening of June 6, when the decoration of the legion of honor was conferred on him.

Dr. Henry W. Farnham, professor of political economy at Yale University, has been appointed Roosevelt professor at the University of Berlin by Columbia University and the University of Berlin.

Professor George A. Hulett, who has been on a leave of absence this year from Princeton University and has been acting as chief chemist of the United States Bureau of Mines, will return to Princeton in the fall, and resume his duties as professor of physical chemistry.

George W. Lamke, assistant professor of electrical engineering at Washington University, and Chester H. Hardy, instructor in electrical engineering, have resigned their positions to take up active practise.

T. T. Waterman, assistant professor of anthropology in the University of California, will spend the summer in New York, in part to study the collections in the American Museum.

At the annual meeting of the American Medico-Psychological Association, which ended its sessions at Niagara Falls on June 13, Dr. Carlos F. MacDonald, of New York, was elected president, Dr. S. E. Smith, of Indiana, vice-president, and Dr. Charles G. Wagner, of Binghamton, N. Y., secretary. The association will meet in Baltimore in 1914.

The eighth annual meeting of the American Association of Museums was held in Philadelphia, from June 3 to 5, 1913, with about one hundred members in attendance. Dr. Henry L. Ward, of the Milwaukee Museum, presided. In addition to the reading of papers the program was so arranged as to provide time for the study of important museum collections at the Academy of Natural Sciences, University Museum, Commercial Museum, Wistar Institute of Anatomy, Academy of Fine Arts and Memorial Hall. The officers elected for the ensuing year are: President, Benjamin Ives Gilman, Museum of Fine Arts, Boston; Secretary, Paul M. Rea, Charleston Museum, Charleston, S. C.; Treasurer, William P. Wilson, The Commercial Museum, Philadelphia. The next annual meeting will be held in Chicago with a supplementary session in Milwaukee.

Immediately after the resignation of professor Willis L. Moore as chief of the Weather Bureau had been accepted by the President, charges were filed against him with the Secretary of Agriculture by responsible men in the service. Those charges, which related to the improper conduct on the part of Professor Moore in connection with his candidacy for the office of secretary of agriculture, were of such a grave nature that the Department of Justice was asked to make an investigation. This investigation has now been practically completed. The facts secured from the preliminary investigation were sufficient to warrant the President in withdrawing his acceptance of Professor Moore's resignation and removing him summarily from the service, which was done April 16, 1913. Charles T. Burns, an employee of the Weather Bureau, was furloughed at the same time, but later reinstated with reduced rank and salary. On June 7, as a result of the investigation by the Department of Justice, six other employees of the Weather Bureau were furloughed without pay, and thirty-one other officers and employees whose salaries had apparently been increased by Professor Moore as a reward for their activities on his behalf were reduced to their former salaries.

Professor Albert Perry Brigham, of Colgate University, will go to Europe in July to remain during the coming academic year. In

his absence Professor Isaiah Bowman, of Yale University, will be the acting secretary of the Association of American Geographers and will prepare the program for the Princeton meeting. Professor Bowman is expected to return from South America in September.

PROFESSOR HOWARD S. REED, of the Virginia Polytechnic Institute, who is spending the year in Europe was a delegate to the Tenth International Congress of Agriculture held in Ghent from June 8 to 12.

ARTHUR H. BLANCHARD, professor of highway engineering in Columbia University, sailed on June 12 to attend the Third International Road Congress, London. Professor Blanchard is a United States reporter on Question 3, "Construction of macadamized roads bound with tarry, bituminous, or asphaltic materials," and Communication 10, "Terminology adopted or to be adopted in each country relating to road construction and maintenance." He has been appointed a delegate to the congress by Columbia University, the American Society of Civil Engineers, the National Highways Association and the American Road Builders' Association.

A SPECIAL research meeting was held by the Royal Geographical Society of London, on June 4, at which Dr. L. A. Bauer gave an account of the progress of the magnetic survey of the oceans and of the chief results thus far obtained. The paper was discussed by Sir David Gill, the presiding officer, by Drs. Shaw and Chree, and by Captain Creak and others.

PROFESSOR H. H. DONALDSON, of the Wistar Institute of Anatomy and Biology, Philadelphia, delivered the commencement address at the St. Louis University School of Medicine on June 5, the subject being "The Mutual Relations of Medical Progress and the Physician."

THREE Chadwick public lectures, on "Nature and Nurture in Mental Development," are being given by Dr. F. W. Mott, F.R.S., at the Royal Society of Arts, on June 6, 13, and 20.

MR. FREDERICK ALBION OBER, an explorer and the author of works on ornithology and travel, died in Hackensack, N. J., on June 1, aged sixty-four years.

PROFESSOR L. G. LEÓN, general secretary of the Mexican Astronomical Society, has died at the age of forty-seven years.

THE death is also announced of Dr. Léon Pervinquière, lecturer on paleontology in the University of Paris.

MR. ARTHUR JAMES has given as a memorial to his brother the income of a sum of 20,000*l.* for cancer research to the Middlesex Hospital, London.

THE Bureau of Standards, Department of Commerce, expects to begin, soon after July 1, the inspection of railroad scales, starting with some of the scales of the eastern roads. This test will be made as a result of complaints from shippers as to the weights charged for by railroads, which have recently drawn attention to the necessity for some governmental supervision over railroad track scales, as well as scales used by shippers doing an interstate business. Every grocer's scale is supposed to be tested as to its accuracy periodically in order that the purchasers may be assured of getting correct weight. In addition, the city or county sealer sees to it that the purchaser secures full weight or measure, and prosecutes violations of the law by the dealers. On the other hand, the railroads annually collect approximately $2,200,000,000 from the shippers of the country on scales which belong to and are operated entirely by the roads, and over which neither the shipper nor the Government has any control or, indeed, any information as to the correctness of the scales. In order to secure information that will enable the Government to draw reliable conclusions as to what shall be done to guarantee the accuracy of railroad weights, and also for the purpose of aiding the railroads to install correct scales, the Bureau of Standards was allowed an appropriation of $25,000 by Congress for the purchase of a test-weight car equipment. The contract for this car has already been let, and it is expected that the car will be delivered soon after July 1, when the appropriation becomes available. The equipment ordered by the

Bureau will differ radically in principle from any in use. Instead of the wheels, brakes, and other movable parts of the car constituting part of the standard weight, as is customary, the Bureau proposes to carry a series of 10,000-pound weights and a truck for moving them on the scales, in a specially designed car provided with a power crane and other accessories for handling the weights. The advantage of this arrangement is that the weights may be standarized and transported from one end of the country to the other with a reasonable assurance that they will remain constant, whereas if the running gear is included in the weight of the test car it would be necessary to verify this weight at frequent intervals. After testing a scale with the standard weights, the empty car may then be put on the scale and its weight determined, after which the weights may be loaded into the car and the scale tested up to the full capacity of the car plus the standard weights.

The eighty-first annual meeting of the British Medical Association will be held at Brighton, July 22–25. According to the *Journal* of the American Medical Association in the section of bacteriology and pathology the papers will include one by Dr. Miller Galt on "The Value of the Blood-count in Obscure Bacterial Infections," and an account by the staff of the John Howard McFadden Research fund on "Some Researches on the Jelly Method of Staining Cells Alive." A joint discussion has been arranged with the section on pharmacology on "Anaphylaxis," the papers promised including a consideration of "The Action of Asbestos and other Finely-divided Substances on Various Physiologic Substances." In the section of climatology and balneology there will be discussions on "Sea Bathing," to be opened by Dr. W. J. Tyson, and on "The International Aspects of British Health Resorts," to be opened by Dr. Neville Wood. In the section of diseases of children, Dr. E. J. Poynton and Dr. Carey Coombs will initiate a debate on the "Affections of the Heart in Childhood." In the section of electrotherapeutics papers and discussions have been arranged on "Röntgen Diagnosis," on "Electrodiagnosis and Electrotherapeutics" and on "Röntgen Therapy and Radium." The subjects chosen for discussion in the section of medical sociology are "Crime and Punishment," on which Dr. Charles Mercier, Sir Bryan Donkin and Dr. James Scott will read papers, and on "Hospitals in Relation to the Stage, the Public and the Medical Profession," in which Sir Henry Burdett and Dr. I. G. Gibbons will read papers. The discussion will, it is hoped, prove valuable in helping to elucidate the problems involved in providing hospital accommodation for insured persons under the national insurance act. A discussion on "Eugenics" will be introduced by Professor Bateson, to be followed by Dr. Stewart Mackintosh, and Mr. Chas. B. Davenport, of New York. The arrangements for the navy, army and ambulance sections include a paper by Major Birrell on "Notes on the Work of a British Red Cross Unit with the Bulgarians."

A British Blue Book has been issued giving statistics of the number of persons killed by wild animals and snakes in British India from 1880 to 1910. According to the summary in the *British Medical Journal* the figures show that the tiger is the animal most destructive to human life; during the last five years of the period it was responsible for 38 per cent. of the total number of deaths caused by wild animals, leopards accounting for 16, wolves for 12 and bears for 4 per cent. Of the total number of persons (2,382) killed by wild animals in the year 1910, the tiger accounted for 882, the leopard for 366 and wolves and bears for 428. Elephants and hyenas, the two other animals distinguished in the returns, were between them responsible for 77 deaths in 1910. Of the 629 deaths attributed to "other animals," 244 are assigned to alligators and crocodiles, 51 to wild pigs, 16 to buffaloes, 24 to wild dogs and 220 to unspecified animals. In 1910 there were 22,478 deaths from snakebite, compared with 21,364 in the previous year, but Bombay was one of the provinces which did not contribute towards the increase, and is in other respects one of the more fortunate parts of India. In Bengal, for ex-

ample, 1,130 persons were killed in 1910 by wild animals and 7,767 by snakes; but Bombay is, with the exception of the Punjab, at the bottom of the list with 22 deaths by wild animals and 1,247 by snakes. The statistics regarding the number of cattle killed by wild animals are not very perfect, but it is estimated that in the five years ending 1910 the number of animals killed was about 100,000, leopards accounting for 48 per cent. and tigers for 32 per cent.

UNIVERSITY AND EDUCATIONAL NEWS

COLUMBIA UNIVERSITY, Rutgers College and the Reformed Church in America receive bequests which may amount to $1,000,000 each as the three principal beneficiaries under the will of Mrs. Mary B. Pell, who died on May 26 at 182 Riverside Drive, and was the widow of John H. Pell. Each beneficiary received a direct bequest of $200,000 and an interest in large trust funds aggregating more than $2,000,000. The bequest to the Reformed Church is for the purpose of building the Wessels Memorial Hall at the theological seminary at New Brunswick, N. J., and the same name is to be adopted for a memorial hall to be erected with the bequest to Rutgers. The fund for Columbia is to erect Pell Hall, in memory of the decedent's husband, who was an alumnus of Columbia.

PRINCETON UNIVERSITY has received $100,000 from Mrs. Russell Sage toward the construction of a dining hall.

GOVERNOR SULZER has signed bills appropriating $450,000 for the College of Agriculture of Cornell University, which also receives $125,000 in the supply bill. The appropriation for the veterinary college is $70,000. A part of the additional appropriation this year is to be used for increasing salaries.

THE bill taking over the College of Physicians and Surgeons by the University of Illinois was advanced to its third reading in the legislature, on June 5, after the defeat of amendments offered by those opposing the project. The state is being asked to appropriate $200,000 for maintenance of the institution, this being a part of the $4,300,000 appropriation asked for the University of Illinois.

THE Plant Industry Hall of the University of Nebraska, containing the departments of agricultural botany, entomology, experimental agronomy and horticulture was dedicated on June 10, the address being made by Professor John M. Coulter, professor of botany in the University of Chicago.

THE following appointments have been made to the faculty of the new school of technology of the Johns Hopkins University: Professor C. C. Thomas, of the University of Wisconsin, to the chair of mechanical engineering; Professor C. J. Tilden, of the University of Michigan, to the chair of civil engineering; and Professor J. B. Whitehead, hitherto professor of applied electricity in Johns Hopkins University, to the chair of electrical engineering.

E. DANA DURAND, former director of the United States Census, has accepted the position of director of the Bureau of Research in Agricultural Economics, at the Minnesota Agricultural College.

PROFESSOR PIERRE BOUTROUX, of the University of Poitiers, France, has been elected professor of mathematics at Princeton University.

DR. R. G. HOSKINS, Ph.D. (Harvard, '10), has been appointed associate professor of physiology in the Northwestern Medical School. Dr. Hoskins, who has been working on internal secretions, will devote three fourths of his time to research and one fourth to teaching.

CHARLES T. KIRK, Ph.D. (Wisconsin, 1911), has been appointed professor of geology in the University of New Mexico. According to the State law of 1909, establishing a Natural Resources Survey, Mr. Kirk becomes ex-officio state geologist, and will spend the present summer in reconnaissance work in that capacity, with headquarters at Albuquerque.

DR. EDWARD C. DAY, Harvard foreign fellow, Naples, Italy, has been elected instructor in zoology, Syracuse University. Dr. M. W. Blackman, of the department of zoology, Syracuse University, has been elected associate professor of entomology in The New

York State College of Forestry, Syracuse University.

At the Alabama Polytechnic Institute, Auburn, Alabama, changes in the staff have occurred as follows: Dr. E. P. Sandsten, professor of horticulture and state horticulturist, resigns to accept a similar appointment in Colorado State College. A. B. Massey, formerly assistant professor of botany and bacteriology in Clemson College, becomes assistant professor of botany. H. N. Conolly, field agent in horticulture, resigns to accept similar work in Colorado State College. Charles S. Williamson, Jr., formerly assistant professor, is promoted to associate professorship in the department of chemistry. Jesse M. Jones is appointed head of the department of animal industry, succeeding Dan T. Gray, who has accepted a similar position in the North Carolina Agricultural College.

The Board of Agricultural Studies of the University of Cambridge, in consultation with the president of the Royal Agricultural Society, has nominated Mr. C. R. Fay, M.A., Christ's College, to be the Gilbey lecturer on the history and economics of agriculture.

DISCUSSION AND CORRESPONDENCE

THE CHARACTER OF THE ENDOSPERM OF SUGAR CORN

In a recent publication[1] dealing with the F_2 generation of a cross between two forms of *Zea Mays*, the one with sugar endosperm, the other with waxy endosperm, the existence of two alternative factors, one for sugar (S), the other for waxy (X) is assumed. Absence of S results in waxy endosperm, absence of X results in sugar endosperm. When both are present a horny endosperm results. The F_2 generation, involving 22,132 kernels, consisted of those with horny, waxy and sugar endosperm in a proportion closely approximating the 9:3:4 ratio.

The assumption of two alternative factors does not meet the requirements, since on this basis a ratio 9:3:3:1 is to be expected. On the basis of Mr. Collins's theory, too, a zygotic construction *ssxx*, involving $\frac{1}{16}$ of the F_2 generation should result in neither a waxy nor a sugar endosperm. Yet the numerical results clearly indicate a sugar endosperm for this portion of the F_2 generation. ("Careful scrutiny of the sweet seeds failed to show any consistent differences that would allow another class to be separated, . . .").

A more plausible explanation suggests itself in an analogy to Cuénot's hybrids between agouti and albino mice. Assuming a basic factor *S*, responsible for the sugar endosperm, a factor *W*, which, acting together with *S*, produces a waxy endosperm, and a modifying factor *H*, which acting together with the factors *W* and *S* produces a horny endosperm, I would suggest for the zygotic constitution of sugar corn *HHwwSS*, and for the zygotic constitution of *Zea Mays* with waxy endosperm *hhWWSS*. On this basis the F_1 generation of a cross sugar × waxy should possess the zygotic constitution *HhWwSS*, which, according to our premises, should result, and in fact does result, in a horny endosperm. Selfing of the F_1 generation should yield the following combinations:

HWS *HWS*	*HwS* *HWS*	*hWS* *HWS*	*hwS* *HWS*
HWS *HwS*	*HwS* *HwS*	*hWS* *HwS*	*hwS* *HwS*
HWS *hWS*	*HwS* *hWS*	*hWS* *hWS*	*hwS* *hWS*
HWS *hwS*	*HwS* *hwS*	*hWS* *hwS*	*hwS* *hwS*

The combinations *HHWWSS* (1), *HHWwSS* (2), *HhWWSS* (2) and *HhWwSS* (4), should result in a horny endosperm, since they contain all three factors; the combinations *HHwwSS* (1), *HhwwSS* (2) and *hhwwSS* (1), should yield a sugar endosperm, since the factor *W* is lacking, and the combinations *hhWWSS* (1) and *hhWwSS* (2) should produce a waxy endosperm, since the modifying

[1] Collins, G. N., and J. H. Kempton, "Inheritance of Waxy Endosperm in Hybrids with Sweet Corn," U. S. Dept. Agric., Bur. Pl. Ind., Circular 120, 1913.

factor *H* is lacking. Therefore horny, sugar and waxy endosperm should be represented in the proportions, actually found, 9:4:3.

To test for the presence or absence of the factor *H*, here suggested, in sugar-corn a cross should be made with homozygous waxy. Three kinds should be found, the first (*HHwwSS*) yielding horny endosperm only, the second (*HhwwSS*) yielding 50 per cent. horny and 50 per cent. waxy and the third (*hhwwSS*) yielding waxy only.

HENRI HUS

UNIVERSITY OF MICHIGAN

THE YELLOWSTONE PARK

TO THE EDITOR OF SCIENCE: In a letter relating to Yellowstone Park which appeared in the issue of SCIENCE for March 21, 1913, there were some statements concerning the experiences which tourists camping out in the park ("sage-brushers," they are usually called) have with the bears, which are certainly astonishing to those who can speak for the sagebrushers if not for the bears. The "cleaning out of sagebrushers' camps by marauding bears" was spoken of as a "nightly occurrence" and it was stated that "three or four sagebrushers are killed nearly every summer in attempting to drive bears out of their camps." My experience as a sagebrusher is that bears will indeed attack the vulnerable part of the camp—the locker containing the store of bacon and the lard can—but even in the vicinity of the Canyon of the Yellowstone, where bears are most numerous, the repelling of an attack on the larder took on much the nature of a midnight sally to rout the neighbor's cow from one's garden patch. There was the same spontaneous rallying against the invasion, the violent laying about with whips and clubs, the resort to loud and picturesque language, and the same clumsy and precipitate retreat of the culprit. Once only we thought it necessary to resort to extreme measures which was to play upon the invaders with a Roman candle. This was completely effective. I would not have a single person miss the great fun and superior advantage of camping out during the tour of the park because of the fear of the bears.

A statement from Lieutenant Colonel L. M. Brett, acting superintendent of the park, under date of April 5, 1913, should certainly reassure all who contemplate a camping trip. I quote as follows:

> As a matter of fact, no tourist or other person has ever been killed by a bear in the park, so far as is known in this office. Our regulations prohibit feeding or meddling with bears, but it is a great temptation for every one to feed them and make pets of them, and the regulations are sometimes violated. Otherwise, there would seldom be any bad bears in the park. As it is, we have instances where the bear becomes dangerous to life and property, and it is necessary to dispose of it. This is done by capture alive and shipment by express to some city zoo, when there is a demand for a bear, and in case there is no demand for it, it is shot. A few instances are on record where people have been attacked and injured by bears. One of these was a tourist; the others were employees of hotels, etc., in the park. In all cases where the facts were known, the person injured was more or less to blame for his own misfortune.

JESSE L. SMITH

THE METRIC SYSTEM OF WEIGHTS AND MEASURES

TO THE EDITOR OF SCIENCE: I agree with all that Professor A. H. Patterson says regarding the greater simplicity and general desirability of the metric system of weights and measures, but there is, perhaps, something that may be profitably said concerning his reference to "those selfish interests which are blocking the way of reform."

Chief among these interests, perhaps, are the machinery-making concerns of the country, and if Professor Patterson were responsible for the conservation of the capital invested in measuring tools, gauges, fixtures, etc., based upon the present system of measurement, and if he believed that a change to the metric system would make it necessary to discard these tools and gauges, he would, I fear, be strongly tempted to object to the introduction of the metric system, notwithstanding his perception of its superiority.

The very general objection of the American machinery constructors to the introduction of the metric system is based upon the belief that the capital above referred to would be thereby sacrificed. A curious feature of the case, however, is that it has been, and can at any time be demonstrated that no such sacrifice would be involved, and therefore the serious objection, which comes from machinery-building interests, to the taking of any step toward the introduction of the metric system, is based upon an entire misconception. This misconception is due chiefly to the fact that most machinery manufacturers do not themselves take the time and trouble necessary to look into this matter, but have taken at par the statements of one or two extreme opponents of the present heterogeneity called a system. These opponents have represented and have made many others believe that the change to the use of the metric system would necessarily mean an alteration in the actual dimensions of machinery now built, and of the tools used in connection with that machinery.

The fact that machinery-building establishments in this country are regularly using the metric system, and are applying it to the identical machines previously made to the English system, without any alteration whatever in the machines themselves, or in the tools for making them, seems to have no weight, and because a matter of dollars and cents is involved, and deep-seated prejudices have been aroused, practically no hearing can be obtained for the manifest advantages of the metric system. The columns of the trade journals are practically closed to all discussions of it, and whenever the subject is up for discussion, by a committee of congress, representatives of, or those professing to represent, the machinery-building interests, will oppose any step taken toward progress in this matter.

Even when it was proposed simply that the various departments of the United States government should make use of the metric system, it was strenuously opposed on account of what it was feared it would lead to, although it was evident that whatever of extra expense might have been involved in the use of the metric system by the government departments would have been borne by the government itself, and would, therefore, have been distributed in such a way as to have been unperceived; even granting that there would have been any extra expense, which is by no means demonstrated and is doubtful.

I mention this matter only to show what the nature of the opposition to the metric system is, and from whence the strength of such opposition comes. It is based upon primary considerations which would be justifiable, or at least quite excusable, if there were any foundation for them. It is my belief that there is practically no foundation for them.

Fred J. Miller

THE TEACHING OF ENGLISH COMPOSITION

To the Editor of Science: I sympathize with Professor Robinson in his concern over the unhappiness of the teacher of English composition, and approve of all he says; but he does not go far enough. He does not answer his own question: "What are we to do to keep him [the teacher] happy in English composition?"

He divides the men in charge of classes in English into two types: the "educator"—who "draws out" his pupils, reads themes less and plays golf more, and from whom the students draw culture in the vaguer sense, a dissemination of sweetness and light—and the "teacher," whose conference work is "confined mostly to grammar, punctuation and the split infinitive; but in the class he finds nothing to do that he considers worth while."

It is a common fault of teachers and of scientific men that they are always trying to divide things, and men, into types and classes, to put them into pigeonholes and label them. What Professor Robinson should do is to try to discover a man who combines the best characteristics of both "educator" and "teacher" and who can not be put in the pigeonhole with either label. If there is no such man to be found, perhaps one can be made. Start with a "teacher" who "hates inexactness and vagueness," and "loves to enforce a clear

intellectual distinction." Make him drop his conferences on "punctuation, grammar and the split infinitive" (I suppose the last two words refer to the latitude of Boston; the rest of the country does not bother itself about the split infinitive) and let his students "criticize their own compositions and those of one another" on these points, as the "educator" does. Let him "read themes less and play golf more," and let him, like the "educator," disseminate the culture of sweetness and light. Give him the right kind of a text-book, with some logic in it, even with the "Barbara, Celarent"—which is a good thing to have in the text-book for reference, although it need not be memorized.

Why should not such a man be found? A teacher, or an educator, like every other man, is the product of heredity and environment, also of habit and of the kind of boss he has—which last may be considered part of his environment. The heredity of the teacher, in Boston at least, is all right; his environment is fairly good, but his teaching habits are bad and he has not been properly bossed; therefore he is unhappy. He is supposed to be teaching English composition, but he is not; he is reading "themes" and correcting errors of grammar and punctuation; he is doing the work that should have been done in the grammar school. "This man of solid thoughtful mind is the only real teacher." Yes, but he is unhappy, and he needs a boss to direct him how and what to teach, and how to "educate," and how to be happy, though a teacher.

Can a boss be found? Why not? Is there not in Harvard some authority that can get the "teachers" and the "educators" together around a table and say to them: "Show us the results of your teaching and educating. Do your graduates have 'mechanical perfection in technique' and there stick; have they style, or do they 'write with the mechanical regularity of one pumping into a bucket'? What proportion of them write even passable English? If the results are not what they should be, get together, you teachers and educators, and plan a better method. If you can not plan one, do as the football players do, hire a coach to plan the method, and let him be your boss until you can show results with it."

"Some day there will be a shaking among these dry bones." Why not now?

WILLIAM KENT

MONTCLAIR, N. J.

UNIVERSITY LIFE IN IDAHO

TO THE EDITOR OF SCIENCE: Permit me to state, in reference to the question of veracity between President James A. MacLean, of the University of Manitoba, and Professor V. L. Kellogg, of Stanford University, that at the request of Professor Kellogg I furnished him with a rather full statement of the facts concerning my recent separation from the University of Idaho. From what I wrote him he prepared his article published by you under the caption "University Life in Idaho." It contains no material statement not furnished by me, and none which I do not at the present time fully believe to be true, notwithstanding President MacLean's denial. In fact, most of the details are matters of common knowledge, which no one could deny in Moscow, Idaho, though it might be done in Winnipeg.

As it is obviously impossible to try the case in your columns, I must be content to assume full responsibility for the essential correctness of Professor Kellogg's article.

J. M. ALDRICH

BUREAU OF ENTOMOLOGY,
WASHINGTON, D. C.,
June 8, 1913

SCIENTIFIC BOOKS

The Space-Time Manifold of Relativity, the Non-Euclidean Geometry of Mechanics and Electromagnetics. By EDWIN B. WILSON and GILBERT N. LEWIS. Proc. Amer. Acad. Arts and Sci., Vol. 48, No. 11. November, 1912. Pp. 120.

Probably the most startling scientific conclusion of the past was the assertion that the earth moved. Even yet, while every one would probably assent passively to this state-

ment, it is doubtful if many persons actually construct their idea of the universe so as to make this motion of the earth a reality. It is not many years since the assertion of Poincaré that the rotation of the earth is a hypothesis, met with as much glee in some quarters as it did astonishment in others. When the average man says that so far as he is concerned, the earth is stationary, he means practically that none of the experiences he has had lead him to think of the earth as in motion. When, however, he becomes a physicist, and tries to harmonize the motion of Foucault's pendulum with his ideas of mechanics, or the deviation of a falling body from the vertical, or the motion of storms across the surface of the earth, he is led to assert as the simplest explanation that the earth must rotate. When he becomes an astronomer and endeavors to reduce the varying configurations of the heavens to some kind of simplicity, he is ultimately led to assert that the earth is moving in space. He is thus brought to consider the question, is the whole universe in motion? and how can its motion be detected? At once he remembers Archimedes's remark: Give me a fulcrum and I will move the world. What is the fulcrum? Here is the trouble. If the earth may be thought of as moving around the sun, the sun may be thought of as moving around the earth. If the mountains and oceans of the earth can turn smoothly around once every twenty-four hours why can not the heavens turn? The problem comes home and must be phrased thus: How can the absolute motion of the earth be proved? Was Galileo right when he said: *E pur si muove.*

We turn first to dynamics for help, but we find that its laws will not avail. The fundamental law is that the rate of change of the momentum of a moving body is proportional to the force acting, where force and momentum are taken as directed quantities. But if we locate the moving particle with respect to an origin which itself is moving uniformly in a straight line we can not detect the fact that the origin is in motion, for the law holds equally well in either case; that is to say, change in momentum is measured by a difference of velocities and can never give us the absolute value of velocity itself. This is the relativity principle of ordinary mechanics. Conversely, if we agree that all we know of the kinetic energy of a particle is that the increment of the energy is measured by the work done by the external force in moving the particle an infinitesimal distance, and that this is invariant for a system whether it is at rest or in motion, with a uniform rectilinear velocity, then all the equations of dynamics are deducible from this basis.

Since dynamics gives us no help, we turn to electrodynamics. There is here a constant which seems to be an absolute constant, the velocity of light. It would seem that if light is a movement or a disturbance in a stationary ether, then we should be able to detect the motion of the earth against this ether. Aberration indeed seems to indicate that we have found our fulcrum. But other experiments seem to show that if there is an ether, it moves with the earth. And the only apparent way to reconcile all the experiments seems to be the assumption of certain laws which make the hypothesis of an ether superfluous. The physicist is here hard pressed for a satisfactory substitute. When the fundamental equations of electrodynamics are examined mathematically, it is found that certain changes can be made in the variables of these equations without affecting the form of the equations. In the new variables the equations read just the same as in the old. That is to say, for certain moving systems the equations are of the same form as for a stationary system. Consequently the quantities involved can only be determined relatively.

The specific statement of the case is this:

Let one end of our laboratory table be our origin, and we will suppose that with respect to an omnipresent stationary observer who appreciates distance and time directly, the origin is in motion in a straight line with uniform velocity, v. The velocity of light we will represent by c, and we will suppose that any velocity can be measured absolutely. This is our first assumption. Then if the fol-

lowing things happen to be facts, we can not ever hope to detect the absolute motion of the table by experiments on light or electricity.

1. If there is a bar AB on the table pointing in the line of the motion, to the stationary observer its length would be only $AB\sqrt{(1-\beta^2)}$ where $\beta = v/c$.

2. The time on a clock at A on the table would read less in the same ratio, that is, if the motion began at noon, when the stationary observer knew that the time was really t the clock would read $t\sqrt{(1-\beta^2)}$.

3. A clock at B could not be made to read the same as a clock at A at the same instant but would be behind that at A by

$$\frac{AB}{v}\frac{\beta^2}{\sqrt{(1-\beta^2)}}.$$

If a clock were instantly moved from A to B the hands would instantly shift through that amount. This is the principle of *local time*.

The stationary observer would deduce at once some very startling conclusions, such as these. If the table could move with the velocity of light, $\beta = 1$, and the length of AB would be nothing at all. The clock at A would cease to register time at all. The obvious conclusion would be that the velocity of light is a maximum that no velocity could ever reach. But even for velocities below that of light we have to give up the idea of incompressible bodies. Energy and mass become confused and physics has to be remade. And the difficulty of time being attached to the place at which we are, so that no time meter could be devised which could be moved around and retain its correct reading, is disturbing. If two clock faces are at the ends of a long axis, and read together when across the line of motion, why should there be a twist in the axis when it is turned into the line of motion?

To enable one to understand these proposed relations of distance and time, Minkowski conceived the notion of giving them a geometrical setting. This is nothing new in physics, for many models have been made to represent various laws and hypotheses. They enable us to look at the relations in a much more direct way; to be able, as it were, to look over a map of the ground. It must be borne in mind, however, that such representations are not substitutions for the thing itself. A temperature-entropy diagram is not steam in a boiler, of course, but only shows certain relations as to the steam in the boiler. So too, Minkowski's geometric setting of relativity is not a picture of the world, but a representation of the relations that are set forth in the theory of relativity.

His suggestion was that if we use a four-dimensional space, measuring x, y, z (which give us the position of the laboratory table) along three axes, and measure on the fourth axis the distance ct, then the fourth axis can be spoken of as a time axis, since c is a constant. In this way we can speak of the situation of the real world at time t as a section in the four-dimensional world by moving a space of three dimensions. The idea is easily illustrated by imagining a wave on a pond made by a stone dropped into the water. The wave spreads out with a given velocity. If now we construct a cone of the proper angle, immerse the point at the center of the wave and let the cone sink at the right speed, the expanding wave will always remain in contact with the cone. Or, so far as geometry is concerned, we can keep the cone stationary and let a cutting plane move upward. The circular section on the plane will then appear to expand like a wave. In an analogous manner we can at least get a phraseology that will describe the ideas underlying relativity of the electrodynamic kind. It turns out that if we represent these in a four-dimensional space the whole statement of the relativity property can be summed up in one simple statement, that is: In the four-dimensional space the choice of our axes of reference is fairly arbitrary. We may take axes inclined at the proper angle to our original axes, as new axes of reference, and the equations for the new x', y', z', t', are just like the original equations. Indeed if we suppose the table mentioned above to move along the x axis, as viewed by a stationary observer, with a uniform velocity v, which we may set equal to $c \tanh \phi$, where $\tanh \phi = \beta$, $\tanh^{-1}$ being the

symbol for hyperbolic tangent, we may write the equations of transformation in the form

$$x = x' \cosh \phi + ct' \sinh \phi,$$
$$ct = x' \sinh \phi + ct' \cosh \phi.$$

That is, if one end of the table, say B, is apparently to the moving observer at a distance x' ahead of the other end, which is the moving origin, A, then the stationary observer knows that the real stationary distance is $x' \cosh \phi$. If the clock at B reads t' to the moving observer, then the stationary observer knows that the time which has elapsed from the beginning of the motion is really $t' \cosh \phi$; and at velocity v, this means that the origin A has moved away from the stationary origin a real distance $vt' \cosh \phi$ or $ct' \sinh \phi$. Hence the real distance of B from the stationary origin is

$$x = x' \cosh \phi + ct' \sinh \phi.$$

Also the stationary observer knows that the clock at B is off from two causes, one its position, at a distance apparently x' from A, which sets it back really by

$$\frac{x'\beta^2}{v\sqrt{(1-\beta^2)}},$$

that is,

$$\frac{x' \sinh \phi}{c}.$$

The other cause is that the time read on the clock since the motion began is t', but the real time as seen by the stationary observer, is $t' \cosh \phi$. Hence we have the equation

$$ct = x' \sinh \phi + ct' \cosh \phi.$$

From these equations the stationary observer could compute x', which the moving observer would think was the distance of B from his moving origin, and the time t' on his moving clock. We have

$$x' = x \cosh \phi - ct \sinh \phi,$$
$$ct' = -x \sinh \phi + ct \cosh \phi.$$

These equations evidently are much like the first pair, and indeed we see that if we change the sign of ϕ, that is, of β, or finally of v—which means that we imagine the moving observer to be at rest and the stationary observer to be relatively in motion—we have the second set. We would therefore expect that if we have two moving observers, with different velocities v and v', we would find similar equations for their respective interpretations of each other's data as to distance and time. Thus indeed if

$$x = x'' \cosh \psi + ct'' \sinh \psi,$$
$$ct = -x'' \sinh \psi + ct'' \cosh \psi,$$

we find x' and ct' to be in terms of x'' and ct'',

$$x' = x'' \cosh (\phi - \psi) - ct'' \sinh (\phi - \psi),$$
$$ct' = -x'' \sinh (\phi - \psi) + ct'' \cosh (\phi - \psi).$$

We see at once from this that the relative velocity is not found by getting the difference of the velocities v and v', but by getting the difference of ϕ and ψ, that is, the relative velocity is

$$\tanh (\tanh^{-1} v - \tanh^{-1} v').$$

After this long preliminary we come to the paper before us which presents a full study of the geometrical representation of these facts, in a most elegant manner. The formulæ above are interpreted as representing a rotation in a four-dimensional space, but not a common space. The rotation in a common space would involve the $\sqrt{-1}$, and to preserve the real numbers as reals, the space chosen is a non-Euclidean space. After all, the difference is really this, that certain terms like *rotation, perpendicular,* etc., do not mean what they ordinarily do, but have meanings related to a given hyperbola, rather than to a given circle. Thus really *perpendicular lines* through the origin are conjugate diameters of a circle whose center is the origin. In the paper "*perpendicular*" still means *conjugate,* but as to a hyperbola and not a circle. This illustrates sufficiently the way in which the terms appear. Only a careful study of the paper itself can give a clear idea of the character of the presentation. The reader simply needs to be on the alert as to the geometrical meaning assigned here to familiar terms whose meaning has been altered.

[1] $\sinh \phi = \beta/\sqrt{(1-\beta^2)}$, $\cosh \phi = 1/\sqrt{(1-\beta^2)}$, $\operatorname{sech} \phi = \sqrt{(1-\beta^2)}$.

The algebraical character of the paper needs a word. Instead of using a coordinate system and ordinary algebra, the authors develop a vector-algebra whose expressions represent directly the geometrical entities under discussion, and which in itself is unchanged by the changes in the axes of reference. This algebra is based upon the notions of Gibbs, and is the same as was developed by Lewis.[1] A rather complete development is given, including the analysis, or differential calculus of these vectors. In terms of the constancy of one of the vectors defined, the vector of extended momentum, the laws of conservation of mass, energy and momentum, are deduced, as well as fields of gravitational force and potential. It is not possible to enter into detail, as the technical character of the developments would demand a large amount of space to do them justice. However, any one desiring a complete and elegant account of the relativity theory, as it is seen in a geometric setting, will find it here. The laws of electromagnetics and mechanics are seen to be theorems in this geometry, which means of course that the representation as a non-Euclidean geometry of four dimensions is not only a fair representation, but is a complete representation of all the facts. It is not to be concluded, however, that it is the only representation; others have been suggested, which do not introduce the notion of a four-dimensional space in the sense it has above.[2] It should be pointed out, however, that the electrodynamic equations remain unaltered if we substitute a distance X for ct and at a time for x given by cT. So that if the universe is four-dimensional and we are moving with the velocity of light in one of the four directions of the fundamental axes, we can not tell which one it is, and indeed it makes no difference. Which means in the end (does it not?) that as we assumed in the beginning that the only thing we could measure absolutely is velocity, therefore, all distances must be expressed as velocities, that is, as times, or conversely, that time as we view it is a distance. Indeed this is the fundamental assumption of the whole theory, that we may never know correctly absolute distance (if there be such a thing) nor absolute time, but we do know correctly absolute velocity.

The memoir is interesting also to mathematicians as a study of a particular non-Euclidean space and the corresponding vector algebra. It illustrates in a very happy way the great simplification introduced into a problem when we apply the proper symbolic analysis.

JAMES BYRNIE SHAW

[1] *Proc. Amer. Acad. Arts and Sci.*, 46: 163–182.

[2] Timerding, *Jahresb. d. Math. Ver.*, 21: 274–285, 1913.

Introduction into Higher Mathematics for Scientists and Physicians. By Dr. J. SALPETER. Jena, Verlag von Gustav Fischer. Pp. 336.

This book has the advantage—as compared with similar previous works—of being written in a very elementary and yet thoughtful fashion. The author has succeeded very well in explaining the principles of higher mathematics in an exceedingly plain way, yet so that he gives all the essential points. For instance, the first three chapters of the book (32 pages or about one tenth of the whole book) are exclusively devoted to a most detailed and elaborate explanation of the three fundamental conceptions upon which higher mathematics are based. These are: (1) the conception of the limiting value of an infinite series of figures; (2) the conception of a function; and (3) the conception of the derivation of a function. To explain the importance and real meaning of these fundamentals the author uses much space, and especially cites a great number of examples from different domains of natural science. In view of the purpose of this work, however, this explanation is not too long. After this introduction only, the technique of differentiating is discussed, also very clearly. Maxima and minima of functions, differential equations, integration, etc., are then explained thoroughly and clearly. At the end of each chapter numerical examples are given, as well as applications to scientific problems. The graphic method is extensively used. As a whole, the book can be recommended to such experimental investigators

who wish to make themselves acquainted with mathematical methods in a limited time. The importance of mathematics for all branches of natural science will certainly increase the more our knowledge progresses and increases in complexity, because it becomes more and more difficult to draw conclusions by non-mathematical reasoning. A book of such character as the one described can certainly therefore claim to be of great importance.

R. Beutner

Rockefeller Institute

Chloride of Lime in Sanitation. By Albert H. Hooker. First Edition. New York, John Wiley & Sons. 1913.

One of the most striking developments in the art of water purification during recent years has been the rapid increase in the use of chloride of lime as a disinfectant. It has been found that astonishing results may be obtained by the use of surprisingly small quantities of this substance. In clear water, such as that of the Great Lakes, the application of eight to ten pounds of this chemical to a million gallons of water is sufficient to destroy practically all of the bacteria. Larger amounts are required for waters which contain organic matter, in some instances nearly one hundred pounds per million gallons being used. Bleaching powder is also being used to some extent in the disinfection of sewage. Here, also, it has an important field of usefulness.

The rapidity with which the use of this substance has come into popular favor is indicated by the publication of the present work devoted exclusively to the use of chloride of lime in sanitation, and consisting chiefly of abstracts of articles published in various scientific journals. Four hundred of these articles are quoted and the essential points of each briefly stated. The author deserves credit for having brought these various papers together. It would be a tedious matter for any one interested in this topic to obtain so much information by his own search. Looking for omissions the reviewer finds that the compilation has been unusually well made.

The abstracts are prefaced with an interesting discussion of the general subject by the author, who gives first a history of the manufacture of chloride of lime and then an account of the method of its use in water purification and for other purposes of general disinfection. In this he is somewhat inclined to minimize the advantages of the use of liquid chlorine. He regards the action of bleaching powder as one of oxidation and does not believe that chlorine acts by itself as a disinfectant in any other way than by liberating nascent oxygen. Some may be inclined to question this. One of the most valuable sections of the book is that which gives directions for dissolving bleaching powder for its practical application. Comparatively little is said in regard to the corrosion of metals by the use of this chemical.

The book is well indexed and will prove an invaluable reference book to sanitary engineers.

George C. Whipple

The Plant Alkaloids. By Thomas Anderson Henry, Superintendent of Laboratories. Scientific and Technical Department, Imperial Institute. Philadelphia, P. Blakiston's Son & Co. 1913.

So long as there is a science of botany, phytochemistry will constitute a perfectly justifiable phase of chemical thought and of chemical investigation. Though for a time, after Kekulé's enunciation of structural chemistry, phytochemistry was looked upon as being not fully up to date as compared with organic synthesis, it is again coming to its own. Since Emil Fischer has pointed out that some of the most interesting problems of organic chemistry are those that are intimately related to biochemistry, phytochemistry has once more become a respectable science even in the eyes of the synthetic chemist.

The present activity in this field is manifested not only by innumerable special researches, but by the rapid growth of book literature. Thus Czapek's "Biochemie der Pflanzen," Euler's "Grundlagen und Ergebnisse der Pflanzenchemie," and Wehmer's "Pflanzenstoffe," which have appeared within a short

period of six years, are indications of the attempts that are being made to bring together between the two covers of a book the modern knowledge in this field.

Most of the recent books, however, are content to present more detailed accounts of restricted phytochemical groups. Next to the literature on the volatile oils, on the fatty oils, and on the carbohydrates, which have received special consideration, no doubt, because of their industrial significance, the alkaloids have attracted considerable attention.

While the fatty oils and carbohydrates represent decidedly restricted groups of chemical compounds, the volatile oils and alkaloids represent much wider fields, chemically speaking. Biochemically, however, these two groups have been regarded as of much less importance than the carbohydrates and fatty oils. This has, however, not lessened their inherent chemical interest, which has always been appreciated. However, their physiological significance has also grown with our increasing chemical knowledge of the compounds of these two groups. Physiological interest is no longer restricted to skeleton-producing or energy-producing materials. It has broadened and by no means to the disadvantage of the science.

It has been said that modern pharmacology owes its existence to the discovery of the alkaloids. Hence one is not surprised to find the pharmacological aspect of the alkaloids receiving consideration even in a treatise that is predominantly chemical. For the same reason the pharmaceutical aspect of the subject has been given due consideration by the author of the book under consideration. Yet there is a purely phytochemical point of view that deserves more careful study than it has commonly received.

That the author of "The plant alkaloids" should follow conventional lines is possibly to be expected. That he himself does not find satisfaction in so doing becomes only too apparent from various statements that might be quoted from his introduction. From a purely chemical point of view, the alkaloids, like all other carbon compounds, should be classified in accordance with the definition that organic chemistry is the chemistry of the hydrocarbons and their substitution products. Thus the conflicts and the irrationalities of a classification based on the so-called typical groups would be avoided. From a botanical point of view, the alkaloids of a family should be considered together, totally irrespective of the nuclei they are supposed to contain. Thus and thus only can genetic relationships be brought out satisfactorily. Such a treatment not only proves satisfactory in the consideration of a single phytochemical group, but it tends to destroy the arbitrary boundaries of these groups.

Nevertheless, we welcome the author's new treatise. It may be claimed that it would be better to revise one of the older texts on the subject. That such revision becomes necessary very often in these days of great research activity is apparent to all who have occasion to use these texts. However, if a new text brings the subject matter up to date, it, as a rule, not only fulfills this important requirement, it is also apt to do more. It usually introduces new points of view at least here and there. For this reason we often welcome a new text rather than the up-to-date revision of an older one.

E. K.

THE TEMPERATURE ASSIGNED BY LANGLEY TO THE MOON

IN his last publication on the lunar temperature,[1] Langley receded from his previous estimate "according to which the soil of an airless planet at the moon's distance

[1] "The Temperature of the Moon. From Researches made at the Allegheny Observatory." *National Academy of Sciences*, Vol. 4, Part 2, Third Memoir, 1889. Two editions of this work were printed. One is said to be "by S. P. Langley," the other "by S. P. Langley and F. W. Very." To prevent misapprehension, I will state that the memoir was the joint work of Mr. Langley and myself. A note written by Mr. Langley, explaining that my name had been omitted from a place on the title page with his own by an oversight noticed too late for correction, was, by some irony of fate, tacked on to the wrong edition, the one which did have my name on the title page.

would have a temperature not greatly above —225° C."[2] On page 197 of that work, a summary of our observations in the lunar spectrum yields the result that "the most reliable spectrum comparisons with a blackened screen show an average 'effective lunar temperature' of +45° C. near the time of full moon." Although the temperature which I had derived from the spectrum comparisons is thus cited, and is the only authoritative figure mentioned, a lingering predilection for his earlier value prevented more than a half-hearted acquiescence in my result on the part of Mr. Langley, who, as chief spokesman, said:[3]

> Contrary to all previous expectations, [the extreme infra-red] nevertheless reaches us, thus bringing evidence of the partial transparency of our terrestrial atmosphere even to such rays as are emitted by the soil of our planet. It is probable, as remarked elsewhere, that even of the heat of arctic ice some minute portion escapes by direct radiation into space. If beyond this we can be said to be sure of anything, it is that the actual temperature of the lunar soil is far lower than it is believed to be; but the evidence does not warrant us in fixing its maximum temperature more nearly than to say it is little above 0° Centigrade.

The last part of this extract refers to the belief that the moon's maximum temperature exceeds that of boiling water, a belief which rested on the opinion of Sir John Herschel and on some thermopile measures with a very large probable error which had been made of the total lunar radiation by Lord Rosse, and which, as is well known, had previously been our most reliable source of information.

I may perhaps be permitted to say that the preceding citation from our joint work did not represent the opinion of the junior contributor. Abbot and Fowle, referring to this memoir, speak of my "most recent revision of the evidence";[4] but this is a misapprehension, inasmuch as I have never revised the material contained in the above memoir. My later publications have been founded on new evidence which is entirely distinct.

Mr. Abbot also says in his work on "The Sun" (p. 311):

> Upon the moon there is no atmosphere and by the observations of Lord Rosse, of Langley and of Very, the moon's sunlit surface falls from about the temperature of boiling water nearly to that of liquid air within the short duration of a total lunar eclipse.

But the preceding quotations prove that Langley never accepted the doctrine of the "hot moon" while he was director of Allegheny Observatory, and that even when pushed by the evidence, he hung back.

Professor W. H. Pickering in 1902 said:[5]

> We do not certainly know the temperature of the moon's surface when exposed to a vertical sun; but according to Professor Langley, it can not be far from 32° F.

N. S. Shaler, in a work published by the Smithsonian Institution in 1903 under Secretary Langley's supervision,[6] said:

> The temperature of the moon has been made the matter of numerous experiments. These, for various reasons, have not proved very effective. The most trustworthy, the series undertaken by S. P. Langley, indicate that at no time does the heat attain to that of melting ice.

Consequently, up to 1903, Langley had not accepted the results which I published in 1898.

[2] *Op. cit.*, p. 193. Langley's original statement in regard to the moon's temperature was founded on what he says ("Researches on Solar Heat," p. 213, 1884) concerning the earth's temperature in the absence of a selectively absorbing atmosphere, namely: "The temperature of the earth's surface is not due principally to this direct [solar] radiation, but to the quality of selective absorption in our atmosphere, without which the temperature of the soil in the tropics under a vertical sun would probably not rise above —200° C." This passage contains a mixture of truth and error. The radiation from a tropical surface at 300° Abs. is 285 times that of a similar surface at 73° Abs. The atmosphere has a protective influence, but not one as extraordinary as this. It must be remembered that Stefan's law was not yet fully accepted.

[3] *Op. cit.*, p. 193.

[4] *Annals of the Astrophysical Observatory of the Smithsonian Institution*, Vol. 2, p. 174.

[5] "Is the Moon a Dead Planet?" *The Century Magazine*, May, 1902, p. 91.

[6] "A Comparison of the Features of the Earth and Moon," Smithsonian Contributions to Knowledge, No. 1438. Part of Vol. 34, p. 6.

Although I have not heretofore attempted to revise the memoir on "The Temperature of the Moon," let me say here that the temperature for which I am responsible in that work (namely, +45° C. on the average), and which is given, indeed, but in such a guarded way as to lose much of its force, is certainly too low. In proof of this, reference may be made to figures 11 and 12,[7] which show the position of the lunar image on the very wide slit which was necessary in order to obtain a readable galvanometer deflection in the almost evanescent lunar spectrum. It will be seen that at no time was the slit completely filled by the lunar image. There were always corners occupied by bits of sky, or by the unilluminated part of the moon, while the blackened screen containing boiling water always completely filled the slit aperture. Consequently, the lunar heat was underestimated. Moreover, the lunar radiation was an average pertaining to regions which include a wide range of temperatures, and necessarily fell much below the maximum radiation from the subsolar point.

In the description of the instrumental arrangements[8] we read:

> Care was taken that the lunar image formed by the condensing mirror, and having a diameter of 6.4 mm., should fall accurately upon the central portion of the slit, and thus only that portion is illuminated. In reducing the observations to a standard width of slit, the variation in this width from night to night having been considerable, the assumption is made that the amount of heat passing through the slit varies simply as the width, which is the same as to disregard the curvature of the upper and lower limbs of the lunar image, as well as the secondary effect of the variation of this lunar semi-diameter.

The inaccuracy of this disposition of lunar image and slit is obvious. The only excuse for neglecting it was that the condition of steadiness of our galvanometer at that time was not such as to call for any greater nicety in the other adjustments. No attempt was made to assign a probable error to the numerical results. They were quantitative, but only roughly so. Hence it has seemed to me a waste of time to attempt to revise these measures. It would be better to repeat them with improved facilities.

Whether Secretary Langley ever accepted the results of the later measures which assign a temperature of 454° Abs. Cent.[9] to the lunar subsolar point is not known; but as he had already stepped up from —225° C. to a temperature "a little above zero," let us hope that he may finally have been willing to go the rest of the way.

To those who find it difficult to accept a lunar surface temperature above that of boiling water, because of the low temperature at great elevations in the earth's atmosphere, where the rarefaction is still much less than on the moon, it may be pointed out that the insolation temperature attained by a planetary surface, after allowing for the variation in the intensity of sunshine, depends mainly on two factors: The duration of continuous insolation, and the absorbent power of the planetary atmosphere for return radiation from the planet's surface. The diminution of solar radiation in proportion to the inverse square of the sun's distance determines the available radiant energy, but the temperature acquired through exposure to sunshine depends to a still greater extent upon the nature of the atmospheric trap by which heat is captured. In this respect the greatest variety prevails among the planets of the solar system. The major planets possess denser and more highly absorbent atmospheres, capable of trapping greater and greater amounts of heat, as their distances from the sun increase. The evidences of heat, namely, strong aqueous absorption-bands in the spectrum, prevalence of cloud, and a vigorous circulation in the atmospheres of planets at so great a distance from the sun, may be explained on these principles, as is shown in my paper on "The Greenhouse Theory and Planetary Tempera-

[7] *Op. cit.*, p. 12.

[8] *Op. cit.*, p. 121.

[9] Frank W. Very, "The Probable Range of Temperature on the Moon," II., *Astrophysical Journal*, Vol. 8, p. 284, December, 1898.

tures,"[10] where, however, the application of the theory to Neptune is purely illustrative.

Mars has a rather rare atmosphere, but a climate of the continental type, giving it warm summers. Quantitative measures of the intensification of the aqueous absorption-bands in the spectrum of Mars[11] confirm the evidence of melting polar snows, and assure us that the summer temperature of Mars is considerably above the freezing point.

The earth, since it is nearer to the sun than Mars and has a denser atmosphere, is, on the whole, hotter than Mars. Terrestrial summer climates would be even hotter than they are, if it were not for the tempering effect of its oceans.

The air modifies surface temperatures both by its absorbent and its convective properties. All parts of the earth's insolated surface are cooled by contact with air in motion. This source of thermal depletion is very much smaller on the moon. On the other hand, the earth's temperature is very much increased by the absorbent action of its atmosphere on telluric radiation, an action which is probably very small on the moon, since its visible spectrum shows not the faintest atmospheric absorption. It is well known that the moon's atmosphere is excessively rarefied, yet a minute amount of some especially absorbent vapor might make a considerable difference in the night temperature, if the absorption-bands were of wave-lengths corresponding to low-temperature radiation. Spectrobolometric observations have not favored the supposition, but are hardly delicate enough to reject it absolutely. Without demanding any exact compensation in these two opposite tendencies, it is sufficient to see that they do oppose each other, and that the final controlling factor is duration of insolation. This is great enough on the moon to permit the formation of a steady subsurface thermal gradient, and the attainment of a maximum temperature depending only on the absorptive coefficient of the surface and the solar constant of radiation. The fact to be emphasized is that no estimates of planetary temperatures are possible without considering the nature of the planetary atmospheres and the duration of insolation, and applying a knowledge of the principles of thermal conduction and of the "greenhouse" theory. But the moon is near enough to permit measurements of its radiant emission, in which the only hypothetical element remaining concerns the explanation of the observed facts. Since Abbot and Fowle, in connection with their objections to my value of the lunar temperature,[12] have referred favorably to the opinions of Dr. W. W. Coblentz, it may be well to point out a few statements by the latter writer which demand reconsideration. Dr. Coblentz in his paper, "Radiation from Selectively Reflecting Bodies,"[13] says:

> The reflecting power of the moon for visible rays, according to Langley, is only 1/500,000 full sunlight. Assuming that at 9 μ the reflecting power is, on the average, ten times that at 0.5 to 4 μ (a low [*sic*] estimate), the value becomes 1/50,000.

Here, "reflecting power," or albedo, has been confused with the total amount of light reflected by the moon, expressed as a fraction of sunlight. I found that the average reflecting power of the moon for solar rays of every wave-length, both visible and invisible, was about 13 per cent.[14] Zöllner obtained, for visible rays, a lunar albedo of 17.4 per cent.[15]

[10] *Philosophical Magazine* (6), Vol. 16, p. 478, September, 1908.

[11] Frank W. Very, "Measurements of the Intensification of Aqueous Bands in the Spectrum of Mars," *Lowell Observatory Bulletin*, No. 36; and "New Measures of Martian Absorption Bands on Plate *Rm* 3076," *Ibid.*, No. 49.

[12] *Annals Smithsonian Observatory*, Vol. 2, p. 175.

[13] *Physical Review*, Vol. 24, p. 314, March, 1907.

[14] *Astrophysical Journal*, Vol. 8, p. 275, December, 1898.

[15] This includes a fraction due to specular reflection which causes the phase-curve for moonlight to differ from that for a diffuse reflector, as well as from the phase-curve for proper lunar radiation emitted from the heated surface (for which see Fig. 15 of my "Prize Essay on the Distribution of the Moon's Heat and its Variation with the Phase," and compare with Zöllner's curve in his "Photometrische Untersuchungen"). If the definition of albedo is restricted so as to include only diffuse luminous reflection, we have such values as the following: Wollaston, 0.12; Bond, 0.071;

Ten times these values would be a reflection of more than the whole and "a low estimate"!

To make my meaning entirely clear, let it be noted that the moon occupies on the average about 1/97,300 part of the hemisphere of the sky, and could send to the earth no more than this fraction of sunlight if it had an albedo of unity, or if it were a perfect diffuse reflector. With an albedo of 17.4 per cent., light from the full moon should be $0.174/97{,}300 = 1/559{,}300$. This is the fraction for which Coblentz adopts in round numbers 1/500,000. A smooth sphere having the property of specular reflection would yield a small star-like image of the sun of great brilliancy, the rest of the surface remaining dark. Nothing of the sort occurs, and the reflection is mainly diffuse; but the notable increase of brightness at, or near, full moon, and the somewhat greater brightness of the limb, as compared with the center, signifies that there is enough crystalline material in the rocky surface of the moon, and especially upon the cliffs which are presented favorably for observation along the lunar limb, to give an appreciable percentage of scattered specular reflections from innumerable crystalline facets. The distribution of such reflection may differ enough from that for a matte surface to account for the peculiarities of lunar reflection. Infra-red rays, on the whole, are less reflected than visible rays by the moon.

Dr. Coblentz finds for his hypothetical quartz moon an emissive power of 0.1 for a limited section of the spectrum near 9 μ; though his Fig. 5,[16] founded on the observations of Rosenthal, makes this fraction nearer 0.25. We may remark in passing that there are no common minerals with a relative emissivity as low as 0.1, even if we confine attention to this limited region of the spectrum, and that the most notable depression in the lunar spectrum at this point is also shown in the solar spectrum and is probably atmospheric. I speak of a "depression" in the emission-curve, but the depression is only a minor feature in what is otherwise a maximum.

Describing his Fig. 7,[17] Coblentz says:

> In Fig. 7, curves *b*, *c*, *d* show several of Langley's observed lunar radiation curves, and as a whole there is a close parallelism between the theoretical and the observed curve, especially at 10.7 μ, where we have to consider only atmospheric absorption.[18]

There is a mistake here. Curve *d*, Fig. 7, is transferred from curve *c*, Fig. 6, which is derived in turn from the hypothetical emission curve with superposed atmospheric absorption. The supposed "close parallelism" vanishes when this mistake is corrected. The principal feature of the lunar curves is that they show a region of maximum radiation between 8 and 10 μ (highest point at about 8.3 μ), where the hypothetical emission curve has a minimum. The mistake is indeed corrected a little farther on where we read (p. 318):

> The computed emission curve is the most intense at 10.2 μ, while the observed curve is the most intense at 8.3 μ.

But here another error is introduced, for we are informed that

> this is to be expected if the observed energy curve is the composite of the selectively emitted energy of the moon and the selectively reflected energy of the sun. The selectively reflected energy of the sun would, to a certain extent, fill up the minima in the lunar emission curve, and as far as our present knowledge goes would explain the observed curves *b*, *c*, *d* [*a*?], (Fig. 7), which lack a minimum at 8.5 μ. As a whole, from whatever standpoint we view this matter, we come to the same conclusion, viz.: that in the region from 8 to 10 μ the energy emitted from the moon consists of its own proper radiation and of reflected energy from the sun.

The explanation, unfortunately for this writer, does not explain, since, as I shall show, the reflected radiation can not possibly exceed about 1/3,000 of the emitted.

At this point in his argument, Dr. Coblentz

Zöllner, 0.1195; W. H. Pickering, 0.0909. Compare Zöllner, "Berechnung der wahren und scheinbaren Albedo des Mondes," *op. cit.*, pp. 161, 162.

[16] *Op. cit.*, p. 317.

[17] *Op. cit.*, p. 319.

[18] *Op. cit.*, p. 318.

introduces the assumption that the absolute temperatures of sun and moon are 5,900° and 350°, and calculates by Planck's formula for the distribution of energy in the spectrum that the ratio of the radiations emitted at these temperatures by complete radiators is 1 : 0.00316.

> This ratio of the emissive power [meaning by this the radiation from equal areas] of the moon to that of the sun [considering the latter to be a complete radiator but the former to have a relative emissivity of 0.1] will then be 0.000316, which is 16 times (0.000316 ÷ 0.00002) the reflected energy of the sun from the moon.[19]

But, as we have just seen, the derivation of the fraction 0.00002 is erroneous, the value assumed for the lunar emissivity is improbable even for a narrow region of the spectrum, and still more so for the entire spectrum, and the argument founded on the supposed lunar "reflected energy" is equally inadmissible, as we shall now see.

When it is remembered that the maximum deflection in the lunar spectrum (furnished by a rock-salt prism) at about wave-length 1 μ in the Allegheny measures was usually not over 10 divisions of a millimeter scale, and that the solar radiation at 9 μ is certainly *not more than a hundredth part of that at 1 μ*, or 0.1 mm., the supposition by Coblentz that any appreciable part of the lunar spectrum at 9 μ, coming indiscriminately from all parts of the lunar surface, can be composed of *reflected* solar radiation, is seen to be preposterous.

The question of specular reflection does not enter here. Isothermal charts of the moon[20] show an entirely different distribution of total radiant energy on the moon's apparent disk from that of moonlight, but this distribution of total radiation is not much altered by the small amount of reflected light which it includes, and is altogether appropriate to that of the emission from a heated body having its highest temperature at the subsolar point. Silicates have an emissive power not very different from 0.9 (nine times as great as this author assumes), or a reflecting power seldom much over 0.1. Throughout a considerable part of the region of proper lunar radiation, the reflected solar spectrum must have been smaller than 0.1, perhaps not more than 0.01 scale division, and it would have been absolutely unrecognizable. The actual deflections which reached upwards of 20 or 30 scale divisions in this part of the lunar spectrum were entirely due to emitted radiation; but the part of the lunar spectrum of wave-length shorter than 4 μ corresponded, both in the form of its energy-curve and in the fraction of its included energy, with the reflected solar radiation.

The supposed similarity between the reflection-curves obtained by Coblentz for some common silicates, and the lunar spectral energy-curve, a resemblance which is by no means conspicuous, is purely fortuitous. The lunar curve owes its shape to alteration by absorption in passing through the earth's atmosphere, and not to local abnormal reflection. The resemblance would have been even less approximate if Dr. Coblentz had drawn his theoretical radiation-curve for the temperature which I have indicated for the moon, which is *not* "300° abs.," notwithstanding that the reader of another work by the same writer, "Infra-red Reflection Spectra,"[21] might infer from a footnote that this temperature rests upon my measurements. The curves published in Fig. 90 of the same work and labeled "Reflection from Moon (Langley," can not possibly have the assigned origin, as is evident from the preceding argument. In repeating this figure in the *Physical Review*,[22] the designation has been changed from "reflection" to a noncommital "radiation," but the quotations cited show that the idea of reflection persists.

Dr. Coblentz also infers from observations

[19] *Op. cit.*, p. 315.

[20] Frank W. Very, "Prize Essay on the Distribution of the Moon's Heat and its Variation with the Phase," Utrecht Society of Arts and Sciences, The Hague, 1891. *Cf.* Figs. 7 to 14.

[21] William W. Coblentz, "Investigations of Infra-red Spectra," Part 4, Appendix 1, p. 114. Carnegie Institution of Washington, 1906.

[22] Vol. 24, Fig. 3, p. 312.

measured in the image of the eclipsed moon that the radiation is reflected and not radiated; but the curves which he has drawn[23] for the eclipse of September 23, 1885, as measured at Allegheny by Mr. J. E. Keeler and myself, are incorrect, since the heat at no time vanished, although it diminished continually until the end of totality; and in other eclipses which I have observed, the heat measured in the image of the eclipsed moon has never been less than 1 per cent. of its value before eclipse, while the diminution of the light is sometimes a millionfold greater; that is to say, there is simply no comparison between the reflected radiation and that emitted by the heated lunar surface during totality. The radiation enormously exceeds the reflection at that time.[24]

I am at a loss to know the source of the statement by Dr. Coblentz that "at the last quarter the heat of the moon is certainly not less than at the full."[25] This statement is totally at variance with all published observations.

One other misapprehension needs to be corrected. It is found in the work on "The Moon" by Professor W. H. Pickering.[26] Professor Pickering says (p. 20):

> The most satisfactory test hitherto made seems to be that of Professor Very (*Astrophysical Journal*, 1898, VIII., p. 266), who compared the amount of heat received from the moon by a bolometer with that received from an equal angular area of sunlit melting snow. The heat was next in each case allowed to pass through a piece of clear glass before reaching the bolometer. The glass allows nearly all the reflected heat to pass, but absorbs that radiated by the body itself. The total radiation in the two cases was about the same, but while the reflected heat was much greater from the snow than from the moon, it was found that the radiated heat was much greater from the moon than from the snow. This means that while the snow is the better reflector, as, indeed, we can see by inspection, the moon is the hotter body. The observation is so direct and simple that it seems impossible to deny the accuracy of the conclusion, but of course it gives us no clue as to what the actual temperature is.

The recognition of the value of this particular observation is all that could be asked, but the really convincing and conclusive experiments with radiating heated minerals, which were performed under identical experimental conditions, and which do give us a "clue as to what the actual temperature is," are not even mentioned in this work, and have been strangely underrated elsewhere.

Professor Pickering goes on to say:

> It would be interesting to repeat Professor Very's observation, comparing the radiation from the surface of the moon with that from the surface of rock illuminated by the sun at temperatures ranging from the melting point of snow to the highest attained by rocks on the earth's surface when exposed to a nearly vertical sun.

Experiments somewhat resembling those suggested, but more instructive, had already been performed.

Another instance of the same misapprehension follows. Abbot and Fowle say:

> Coblentz has lately shown that some of the materials likely to be prevalent on the moon's surface are very poor radiators at such temperatures as these, and this would tend to explain why Very has found a temperature so much higher than that of a "black body" under similar conditions.

They also remark:

> We do not know what its surface is composed of, and therefore have no means of discovering the relations which connect the lunar temperature and radiation.[27]

The opinion that "the moon is probably a very bad radiator" is also reiterated by these authors in a recent article,[28] where are some personalities to which I need make no further allusion, as they only obscure the real question.

[23] *Physical Review*, Vol. 24, Fig. 1, p. 310.

[24] Compare Frank W. Very, "The Temperature of the Moon," *Astrophysical Journal*, Vol. 24, p. 354, December, 1906.

[25] Carnegie Institution publication, p. 112.

[26] Published by John Murray, London, 1904.

[27] *Annals Smithsonian Observatory*, Vol. 2, p. 174.

[28] *Astrophysical Journal*, Vol. 25, p. 95, March, 1912.

The language of these quotations implies that I have not considered the radiating power of the actual lunar substance, but have assumed an ideal moon; whereas the truth is that extensive observations were made on the radiations from heated silicates and other substances of which the moon's surface is liable to be composed, and comparisons were instituted with these, and not with an ideal radiator, before reaching a final conclusion. The result of this comparison of the radiant behavior of various materials is important, since I find[29] that, given a sufficient duration of insolation to enable a steady state to be reached, there is not much difference in the emission from various materials. Thus a very poor radiator, such as rock salt, radiates from a great depth of its interior substance, and the summation of radiation from many interior layers compensates for the small emission from any one layer. Hence it is not correct in such cases to state emissivity as a surface function. The complete statement of emissivity must be a volumetric one and must include the subsurface thermal gradient.

Ferrel showed on theoretical grounds that the law connecting temperature and radiation for the moon may be expressed as an equation of condition, where, if the coefficients of radiation and absorption of the same substance are always equal, it makes no difference what the substances are; all will eventually reach the same temperature. Some exception to the theory must be made for such substances as ice which are kept cool by melting and evaporation in sunshine. In his actual illustration, Ferrel used the law of Dulong and Petit, but any other formula may be substituted as far as the principle in question is concerned. The important point is that

> the same results would be obtained sensibly with any ordinary conductivity for heat if the same side of the moon were permanently exposed to the sun, for the temperature gradient by which the heat would be conducted inward would soon become so small, in this case, that the rate by which heat would be conducted inward would be insensible, as in the case in which heat is conducted outward from the interior of the earth.[30]

In a different category from the books and articles already cited come such works as that by Fauth.[31] A footnote on page 26 of this book refers to my writings on the moon, but the author does not appear to have read them carefully. On page 139 he says:

> Lord Rosse was enabled by his measurements to appreciate the differences in temperature on the moon's surface during full radiation and by night, and found them to be over 300° C. But the temperature can not be determined with any accuracy. Lord Rosse's results have often been questioned, but they are supported by the recent investigations of Very. Very believes that at the moon's equator, when the sun is at its highest, the ground increases its temperature by more than 100° C. (which would be —173° C.).

This is pretty nearly a hopeless case. I am sure that neither Lord Rosse nor I could recognize our own work in the conclusions attributed to us, where absolute temperatures and temperatures on the centigrade scale are mixed up indiscriminately, in spite of carefully guarded language in the original sources, and where the opinion is hazarded that the *maria* are frozen oceans—a supposition which is completely overthrown by the thermal measures.

The selection of these quotations for special mention does not imply that there are not others equally objectionable in the literature of the subject.

My chief reason for wishing to call attention to the imperfect conceptions of one whose splendid contributions to science condone all minor imperfections is because Langley's early and gradually changing opinions on the subject of lunar temperature still act as a barrier against the acceptance of conclusions which are founded on reliable observations. This will be quite evident from the passages

[29] "The Probable Range of Temperature on the Moon," I., *Astrophysical Journal*, Vol. 8, pp. 199–217, November, 1898.

[30] William Ferrel, SCIENCE, Vol. 6, p. 542.

[31] "The Moon in Modern Astronomy," by Ph. Fauth, with an introduction by J. E. Gore, F.R.A.S.

cited from the treatises of Shaler and Pickering.

In conclusion, let me remark that even though we can not go to the moon to see for ourselves what its temperature may be, or whether gravitation acts there as here, or what may be the power of the sun's unabsorbed radiation, we are gifted with reason and can form for ourselves just conclusions from observed facts. Otherwise all astrophysics would be impossible.

FRANK W. VERY
WESTWOOD ASTROPHYSICAL OBSERVATORY,
May, 1912

THE ORE DEPOSITS OF WESTERN UNITED STATES

AMONGST the valuable publications issued by the United States Geological Survey is Mr. James M. Hill's Bulletin 507 with the misleading title "The Mining Districts of the Western United States," as it deals solely with the metalliferous mining districts. These districts are grouped and numbered in each one of the 13 states considered, and their distribution is shown upon 14 maps. The text gives for each district its chief rocks and metalliferous products, the publications of the Survey relating to each one, and the distance and direction of the nearest railroad station, etc. A full index of all the districts mentioned concludes the work, which should be in the hands of every one interested in the ore deposits of the west.

To the general student of metalliferous deposits probably the most instructive portion of the bulletin will be the "Geologic Introduction," by Professor Waldemar Lindgren, late chief geologist of the Survey, present head of the Department of Geology of the Massachusetts Institute of Technology, and one of our foremost mining geologists.

The evidences of the mineral wealth of the Cordillera are found extending territorially from the Pacific shore of United States eastward to western Texas and Oklahoma, and geologically from the Pre-Cambrian to the Recent.

Since the deposition of ores is due to geologic agencies, it is pointed out that in the Cordilleran region, where the rocks are horizontal and undisturbed, the ore deposits are missing or rare and poor. Again, while the metallites occur mostly in the mountain ranges, yet many, even of the highest, are barren; showing that without other conditions, uplift, faulting, and crushing of the rocks, and the circulation of water through them does not always produce ore deposits.

Characteristic important deposits are where Paleozoic sediments have been traversed by moderate-sized eruptive masses of Cretaceous or Tertiary age, the ores apparently being deposited shortly after the intrusion. A less common but often rich deposit is found in Tertiary andesitic and rhyolitic flows.

All these deposits are believed to have been formed by water solutions—largely in fissure veins, chambers, and impregnations. The ores, except gold and its tellurides, were apparently originally deposited as sulphides of lead, iron, zinc, etc., or oxides of iron; but down to or below the permanent water level, which varies from a few hundred to 2,200 feet, these sulphides have been oxidized to cerussite, hematite, calamine, etc. Just below the oxidized zone occur secondary sulphides, like chalcocite and silver-bearing minerals, concentrated by the percolating waters, often into bodies of great richness.

Professor Lindgren gives under each state a more detailed summary, but space does not allow us to continue further, and any one interested can procure a copy by writing to the director of the Geological Survey at Washington.

Without intending to be captious it is suggested that in future editions the term "ores" should be used for "metallic ores" (see pp. 7–9), because there are no ores that are not metalliferous. In the same way "mineral deposits" ought not to be used for metallites or metalliferous deposits when the author intends to exclude the memetallites or non-metalliferous deposits (see pp. 5–9).

M. E. WADSWORTH
UNIVERSITY OF PITTSBURGH

SOCIETIES AND ACADEMIES

NEW YORK ACADEMY OF SCIENCES
SECTION OF GEOLOGY AND MINERALOGY

On December 2, 1912, Professor J. Edmund Woodman, chairman of the section, called the meeting to order at 8:25 P.M., in the west hall of the American Museum of Natural History.

No regular business was transacted, but the whole time given over to a lecture on "The Seismograph and What it Teaches," by Professor Harry Fielding Reid. Professor Reid explained the principles of construction and some of the differences in seismographs, using an extensive assortment of lantern views. The lecture was very instructive, and aroused especial interest because of the recent installation by the New York Academy of Sciences of a new seismograph in the American Museum of Natural History, where the section holds its meetings. At the close of the lecture opportunity was given to inspect the new instrument. About 150 members and visitors were present.

Charles P. Berkey,
Secretary of Section

On January 6, 1913, immediately following the adjournment of the regular business meeting of the New York Academy of Sciences, Professor J. Edmund Woodman, chairman of the section, called the meeting to order in the usual meeting place in the American Museum of Natural History, at 8:25 P.M.

Professor D. W. Johnson delivered a paper on "The Shoreline of Cascumpeque Harbor, Prince Edward Island." After explaining with the aid of blackboard maps and sketches the criteria for distinguishing between real and apparent oscillations of shorelines, Professor Johnson further illustrated the discussion with lantern views, amplified where necessary by means of diagrams thrown on the screen. He concluded that the area under discussion is probably the best example of features normally produced by subsidence of a maturely dissected plain to be found on our Atlantic seaboard. He finds no evidence, however, that indicates subsidence in geologically recent times—that is, within the last 2,000 years. In the questionnaire following, the speaker presented still other evidences strengthening his conclusions as to the duration of stable conditions, and made brief references to other localities along the Atlantic coastline of North America.

The program was concluded by brief notices of important papers given at the meeting of the Geological Society of America, at New Haven, Conn., December 28–31, 1912. Ten minutes were devoted to each of the groups, paleontology, economic geology and petrology, by Professor A. W. Grabau, Professor James F. Kemp and Charles T. Kirk, respectively. The meeting, though technical, was attended by some fifty persons.

Charles T. Kirk,
Secretary of Section

On February 3, 1913, the chairman of the section, Professor J. Edmund Woodman, called the meeting to order in the west lecture room of the American Museum of Natural History, at 8:30 P.M., and introduced Mr. F. H. Newell, director of the U. S. Reclamation Service. Under the title of "Home-making in the Arid West" the speaker delivered an extremely interesting and instructive lecture on the problems of irrigation in our arid and semi-arid regions. He showed how the United States irrigation engineers must be able to handle a manifold situation. In many instances the determination of flood water possibilities, the areal survey of the project, and the installation of the dam are coupled with such other considerations as soil surveys, building and running a cement plant, constructing and managing a railroad for passenger- as well as freight-traffic, generating and subletting electric power from the flood water spilling over the dams, providing for workmen in isolated settlements—even to furnishing them amusements in the way of motion picture shows—dealing with Indian tribes to the extent of inducing the men to work; all these and other institutions and functions being owned and controlled by the Reclamation Service of the United States government.

To carry out the various projects requires the expenditure of some twelve million dollars annually, or about a million a month. When the score or more of projects are all completed, homes on the farms and in the villages of the arid west will be provided for more than two million families.

The fallacy of dry farming was clearly shown by the loss of about one crop in three through such practise.

The lecture was splendidly illustrated with polychrome slides of very characteristic western views. Owing to unpleasant weather, but 75 persons attended. The audience was further entertained by Director Newell's informal replies to questions from members and visitors after the formal presentation of the subject.

Charles T. Kirk